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ORIGINAL LECTURES.

ON THE TREATMENT OF TYPHOID FEVER.

A Clinical Lecture.

BY PROF. DUJARDIN-BEAUMETZ,
OF PARIS.

(Continued from page 324.)

GENTLEMEN: I pass now to the proper medical treatment of the disease. I shall commence by reviewing the different kinds of treatment which have been in vogue; then we shall examine the approved medicinal measures, and the indications for their employment during the regular course of the fever and during the complications.

The different kinds of treatment of typhoid fever may be arranged in three principal groups. In the first, it is the fever alone which demands attention; in the second, it is the typhoid poison or miasm which is to be combated; in the third, it is empiricism which has directed the treatment. Hence we will examine successively the antipyretic treatment, the parasiticide treatment, and the empirical treatment.

We find in the antipyretic medication the same divisions as in the previous chapter, viz., we shall have to examine successively refrigerants and antipyretics. Refrigerant medication has found in the treatment of typhoid fever one of its principal applications, and we shall have to study here, cold baths, tepid baths, refrigerant apparatus, cold affusions, and lavements.

There is no question in therapeutics which has of late years provoked more vehement discussion than that of the application of cold baths to the treatment of typhoid fever; and this method to which Brand has justly given his name, for he it is who has furnished it with a rigorous and mathematical formulation, has been attacked and defended by resolute adversaries and determined partisans. I myself have taken part in this debate, and I am going to set forth as impartially as possible the advantages and disadvantages of this method.

Brand has thus formulated the treatment by cold baths. "You must administer baths of 64° to 68° F. of fifteen minutes' duration, from the fifth day of the fever; these must be repeated day and night every three hours, as long as the temperature of the rectum exceeds 102° F." In applying rigorously this method, so simple in appearance, Brand considered himself warranted in affirming, *that every case of typhoid fever treated regularly from the beginning by cold water, will be exempt from complications, and will get well.*

I shall not here enter into the details of Brand's method, details which pertain to the manner in which the patient should take the bath; and I refer you in this regard to the minute directions which Dr. Chapuis has given in his thesis (Paris, 1883), and which are founded on the technics in usage in the hospitals of Lyons. Lyons is, in fact, the only city in France where Brand's method is used in all its rigor, under the persevering efforts of Dr. Glenard, who, during his captivity at Stettin, had an opportunity to witness the results which Brand was obtaining. Dr. Glenard must congratulate himself on the success which has attended his persevering efforts, for we see almost complete unanimity in the hospitals of Lyons in reference to this refrigerant medication.

It is upon statistics that the partisans of this method base their arguments almost exclusively. Jaccoud, from a review of more than eighty thousand cases, arrived at an estimate that the average mortality in typhoid fever was 19.23 per cent. On the other hand, taking as a point of comparison the military hospitals, and in reckoning under a common title the continued fevers and typhoid fever, a mortality of fourteen per cent. is obtained. The application of Brand's method has caused a considerable fall in the percentage of deaths, since, out of thirty thousand cases treated by cold baths, there was a mortality of only nine per cent., and in the case of the military hospitals, of only seven per cent., or even less; and if we may rely upon the figures furnished by Abel, Physician in Chief of the Second Corps of the Prussian Army, in Pomerania, the mortality was reduced to nothing.

What is the real value of such captivating figures as these? You know already my opinion concerning statistics as applied to therapeutical results—I expressed them very clearly while on the treatment of pneumonia—I have not changed my mind, and I persist in considering statistics as an unreliable criterion of the results of our treatment, because, in order that this method may give positive results, it is essential that the individual cases which are collected and compared together shall be proper subjects for comparison, a thing which does not happen in pathology. Take, for example, typhoid fever. Do you believe that one typhoid patient is exactly like another typhoid patient? The age of the patient, the state of his vital forces, the relative severity of the epidemic, the period of the year, the nature of the locality even, have a great influence on this pathological aggregate, and modify its march and its fatality. It is here especially that we see the influence of what I have called the morbid genius of epidemics, where one sees epidemics relatively benign, succeed epidemics that were malignant; and according as you apply the same method of treatment to the first or to the second, you will have multiplied successes or almost constant failures.

The arguments, Gentlemen, which I adduce against statistics, I can in turn obtain from statistics themselves, and to the figures so convincing of Brand, of Glenard, of Strube, of Abel, etc., I need only oppose those of Schmidt of Erlangen, of Goltdammer of Berlin, of Boudet of Lyons, which show us that by rigorously employing the cold-bath medication the rate of mortality is considerably increased, approaching, as it does, a mean of nineteen per cent., and even exceeding this sometimes. I might even compare the other methods

of treatment with that of Brand, and show you also by figures that the refrigerant method has nothing to boast of.

I know well the objection which the partisans of the cold-bath cure make to these last statistics. They tell us that in these cases Brand's system of treatment was not carried out with mathematical precision, and according to his rules, and what is of more importance, it was not employed from the very commencement of the disease. My reply to this objection is more formal, and I assert that in the majority of cases, and outside of military practice—for which I make some reservation—the method of Brand is impracticable, both in our hospitals and in private practice. Not that I believe that we cannot in our hospitals and in our ordinary practice succeed in giving, despite the difficulties which attend them, cold baths day and night to our patients, but because it is necessary, according to Brand, to give these baths before the fifth day of the disease. Never do patients enter our hospitals at this period, and when they do enter, as we cannot be sure of our diagnosis till after the seventh day, it is not till this date that the cold-bath treatment can be commenced.

To this objection, Brand's partisans have replied categorically, and Bard has given the reply in this concise form: "In order that the method of Brand may have full efficacy, all fever patients should, without distinction, be subjected to the bath; *the bath will know its own.*" I do not know if the bath will "know its own" or not; but what I do know is, that we cannot, without danger, subject all our fever patients to a rigorous and severe method, and when we are ignorant of the cause even of the febrile process. Do you believe that the cold bath may not determine profound congestions? Do you believe that you can thus, without danger, force the circulating blood from the cutaneous surface to the central parts? My colleague and friend, Dumontpallier, has well argued that the slow and progressive refrigeration does not provoke visceral congestion; but to the clinical and experimental facts to which he appeals, I have opposed experiments and observations absolutely contrary to these views.[1] Brand's method, whatever one may say, is not free from danger, and not a few of the complications which supervene on the part of the lungs or intestines, in patients submitted to this treatment, may be attributed to it. As for the pulmonary liabilities, there is no doubt that as a result of the cold-bath medication congestions and inflammations of the lungs have arisen; and during the discussion which took place in 1876 and 1877, in the Society of the Hospitals, this fact greatly impressed the majority of those who took part in these debates.

I must say as much as regards intestinal hemorrhage; I believe that in certain cases the cold baths may favor these hemorrhages in patients who are predisposed. These complications, moreover, on the part of the lungs and intestines, are quite explicable from the very action of the cold baths which are employed.

Where do you suppose the blood goes to which circulates at the periphery when it is driven inwardly by the vaso-motor constrictive action of the cold bath and ice-water lotions? The blood cannot find refuge except in the internal organs, and this reflux cannot but favor congestion of the abdominal viscera. Soulier has made many experiments on animals to show that the vaso-motor constriction is produced almost at the same time at the surface and in the central organs; but I believe that these experiments, however careful they may have been, place the animal in quite different circumstances from those of our typhoid patients.

To sum up, I consider the exclusive method of Brand, and the rigorous and mathematical rules which he has formulated, as deserving to be banished from the treatment of typhoid fever, and for this reason especially—because it requires, in order to derive from it all the results which it promises, to be applied before the diagnosis can be certain; for, employed later, this system only gives—according to the acknowledgment of its most zealous partisans—results comparable with those of other therapeutic methods, and with greater danger to the patients. You will see, in fact, that we have for the treatment of certain manifestations of abdominal typhus, hydro-therapeutic means much less dangerous and quite as powerful—not, perhaps, from the point of view of hyperyrexia, but from that of the other symptoms of typhoid fever; for, in my opinion, the advocates of the refrigerant medication have committed an error in vociferating, "The hyperpyrexia! behold the enemy!" The hyperpyrexia, as Peter has well said, does not constitute all the danger of the disease, of which it is only one of the manifestations.

Tepid baths are, in my judgment, much more applicable to ordinary cases of this disease, and I pointed out several years ago the advantages which might be derived from these baths, which, as my hospital experience has shown, have an antipyretic action much less marked than that of cold baths, but are absolutely safe. They allay the excitement of the nervous system, diminish the sensation of burning heat, and enable us to maintain perfect cleanliness in our patients.

There are two ways of administering tepid baths. Some, as Ziemssen, Schutzenberger, and Laure, of Lyons, employ baths of decreasing temperature, and from 95° F. they lower the temperature of the water, in the course of ten minutes, to 77° F. Others, and I am of their number, keep the warm bath at a constant temperature of five degrees below that of the patient, to wit, varying from 93° F. to 98° F. These baths should be of much longer duration than the cold baths, and without going as far as Reiss, who keeps the patient in them almost all the time, I am of the opinion, relying especially on the experiments of Thery, that it is sufficient to prolong them from twenty to thirty minutes. These baths are repeated once or twice a day, the attendant having care to sustain the forces of the patient by giving him, during the bath, wine and meat broths. I have always seen, under the influence of these tepid baths, typhoid patients with hyperpyrexia and delirium manifest a notable amelioration of the symptoms, become calm, and obtain quiet sleep.

I shall say little about the employment of refrigerant apparatus, not because I doubt their value—on the contrary, they are the only means which enable us to apply, systematically and rigorously, the refrigerant method—but because I have had no personal experience with regard to them. They are, in fact, quite

[1] Dumontpallier, on the Refrigerant Method. Gaz. des Hôp., March 13, 1883. Dujardin-Beaumetz on Refrigerant Medication, Un. Méd., 1883.

complicated and costly, and this is the reason why no one but their inventor, my colleague, Dumontpallier, has applied them in practice. If we refer to the conclusions which he has recently formulated, these apparatus have given him good results. We find means much more easy of application in cold affusions, lotions, and the wet pack, which can be used by anybody.

Affusions of cold water, or rather of sea-water, which was Currie's treatment of fevers, are no longer in use, and the cold lotions of which I have already spoken are far preferable. These lotions of cold water are decidedly antithermic in their action, as no one who has watched their effect can have failed to observe; the temperature falls appreciably under their use, especially if they are repeated several times a day. This heat-reducing power is, I well know, limited, and in certain grave cases of ileo-typhus these lotions do not suffice to bring down the temperature, and we must have recourse to more active means. I believe the refrigerant action of these lotions to be a secondary effect, the principal and dominant action being that which they exert on the vaso-motor functions of the cutaneous capillary network, functions so profoundly disturbed in typhoid fever, and which tend to become restored under the influence of these cold lavations or spongings.

This vaso-motor and revulsive action is much more energetic when the wet pack is used. The method is very simple; the patient, in a state of nudity, is wrapped from head to foot in a sheet or blanket wrung out of ice-cold water. It is well, as a preliminary step, to have a rubber blanket spread upon a mattress; over this you place the wet sheet, in which you wrap your patient. Liebermeister advises that this envelopment should be continued for ten minutes; for my part, I prefer a shorter duration of a minute or so, after which the patient is taken from the wet sheet and removed to his bed. If I prefer wet wrappings of short duration, to the practice of Liebermeister, it is because I do not wish to obtain refrigeration from these envelopments, but only a *regulative modification* of the nervous system, and this effect will be the more marked, the shorter the duration of the cold application. This is, Gentlemen, one of our most powerful modes of treatment in cases of typhoid fever of ataxic and adynamic character, and you will derive great benefit from it. Foltz has recently added cold lavements to the refrigerant medication. These lavements of water at 50° F., lower the temperature of patients—in a feeble manner it is true—but still appreciably, and this is a fact worthy of being remembered. So whenever you have occasion to give enemata to typhoid patients, see to it that the water which you use is cold.

Having finished refrigerants, I pass now, in accordance with the order which I adopted in the previous lecture, to bloodletting.

Very much in use in the treatment of fevers at the commencement of this century, and before we had precise notions respecting the nature of typhoid fever, bloodletting was still practised at a later date in this disease by Louis and Chomel, and especially by Bouillaud, who thought to *jugulate* the disease by applying to it the formula of "bleeding upon bleeding" (*coup sur coup*). To-day the practice of bloodletting, local or general, is completely abandoned; and, nevertheless, whenever nature herself proceeds to the letting of blood, and when these losses do not exceed certain limits, we see the temperature fall, and symptoms of the greatest gravity abate; so that, in a great many cases, intestinal hemorrhages are an element of prognosis rather favorable than otherwise. The two clinical charts which I here show you, indicate well the antithermal action of these hemorrhages. It is true that you sometimes purchase this amelioration by a long and painful convalescence; however this may be, we note the fact without venturing to authorize the interference of the physician in order to produce artificially, in typhoid patients, loss of blood.

I now come to the study of medicaments which act on the fever by the intermediation of the nervous system, and we shall examine successively the treatment of typhoid fever by digitalis, aconite, veratrum viride, and sulphate of quinine.

It is Hirtz who, in our country, has defended with the most ardor the treatment of this fever with digitalis, a practice already adopted since 1862 by Wunderlich. Digitalis is given in the form of infusion, and fifteen to thirty grains of the powdered leaves are steeped in a gill of water, all of which is administered daily in divided doses. These doses, continued for three days, cause a notable diminution of the pulse and temperature, but despite this powerful antithermic effect, which is not denied by any observer, this kind of treatment is not much in favor. One fears, with reason, the action of digitalis on the heart, which is so often affected, as we have seen, with symptomatic myositis, as well as the emeto-cathartic effects which digitalis determines when given in such large doses.

I shall not take up your time with the treatment by aconite recommended by Lavasseur and Deshayes, of Rouen, nor with that of veratrum viride employed by Hirtz, Vogt, and Liebermeister, their trials not having been since repeated; at the same time remarking, *à propos* of the first of these medicines, that if you wish to experiment with aconite you should use the tincture of the root and not that of the leaves, the latter having no medicinal virtue, and I pass now to the study of a kind of treatment which has numerous partisans at home as well as abroad. I refer to the medication of typhoid fever by quinine.

Broqua, of Mirande, in 1840, communicated to the Academy of Medicine the good results which he had obtained in the treatment of typhoid fever with large doses of sulphate of quinine, and we have seen successively Chappotain, of St. Laurent, Pereira, Boucher, of Ville Jossy, Blache, Briquet, and Monneret boast of the effects of this medication. Monneret went the furthest in this direction, for endeavoring to substitute for poisoning by the typhogenous miasm the toxic effect of quinine, he was in the habit of administering as much as seventy-five grains a day of this medicine. Such bold experiments were sometimes followed by bad results; therefore this medication was abandoned, at least in our country. Vogt, in 1858, and Wachsmuth, in 1863, and, lastly, Liebermeister, in 1867, repeated these first trials, but this time they applied to the study of the effects of the medicament the use of the thermometer, and gave precise indications of the administration of the medicine. This treatment was rapidly adopted in foreign lands and in France, and we see Lindwurm, of Munich, Œffner, Larsen, of Copenhagen,

Pawer, Kaulich, Jaccoud, Germain Sée, Herard, Barthez, and others, put in practice this medication.

It is the sulphate of quinine that is most often employed; yet in Germany use is made of the hydrochlorate, and in France Jaccoud prescribes the bibromhydrate of quinine. These salts are administered in solution, or oftener in capsules; the pill form should be discarded, for it not seldom happens that on account of the state of the digestive tube, these pills pass through the intestinal canal unaltered. But the capital point, and it is this on which Liebermeister has insisted, is to give large doses; for example, you should make your patient take eight grains every fifteen minutes till half a drachm has been reached. Liebermeister often exceeds this quantity, and continues giving the small doses every quarter of an hour till from forty to sixty grains are administered, but in this country we generally stop at half a drachm.

The period of administration of these massive doses has also a great importance. Liebermeister counsels to give the quinine between five and seven o'clock in the evening; Germain Sée prefers the morning; Jaccoud, with good reason, says that you should give it in the morning or evening, according to the effect which you desire to obtain. Do you wish, for instance, to procure a lowering of the evening temperature? give your quinine in the morning; Do you wish to obtain a matinal fall? give it in the evening. Leibermeister and Kaulich give one large full dose on one day only, and do not repeat the dose unless the temperature takes on again an ascending march. Jaccoud gives his salt of quinine in decreasing doses for three days. Sée administers it without interruption. I believe that the method by interruption has great advantages over the continuous employ of the medicament, and you ought to be guided in this regard by the thermometric curve.

Employed after this fashion, sulphate of quinine produces in typhoid patients a very pronounced depression of the pulse and temperature, which lasts often for two days, and when the thermal curve rises it does not attain as high a point as before the exhibition of the quinine. But this antipyretic action, obtained with such large doses of quinine, has certain disadvantages. In giving to the patient thirty, and often forty or more grains of quinine, you are likely to overstep the therapeutic effect, and obtain the toxic action on the brain and on the heart. Germain Sée and Rochefontaine affirm that this cardiac action is of a tonic kind, but Laborde, with much reason, maintains that it is dangerous in hearts with degenerated muscular fibre, and you know that the latter is a very common complication in the infectious diseases. Moreover (and this is an argument which I adduced in the last discussion at the Academy), the typhoid patient is a bad subject for treatment, not only because absorption of medicines is difficult in consequence of the unhealthy state of the digestive tube and the lymphatic vessels, which originate there, but also because the functions of the kidney and the liver are notably compromised. In a former course of lectures I showed you the capital importance of the liver and of the kidney from the point of view of the action of medicines, and from all these considerations you see how easy it is, when drugging your typhoid patients, to exceed the therapeutic and obtain the toxic action of your remedial agents.

You ought always, Gentlemen, to have these facts in mind when prescribing active medicines in large doses to your patients in this fever. And while recognizing the benefits of quinine medication, I believe that it is best to be very prudent in the administration of this alkaloid, and never to exceed the dose of thirty grains a day, and always to have care not to give this medicament continuously. Therefore, I much prefer, as an antipyretic, salicylic acid to quinine, because one obtains antithermic effects quite as powerful with the former, and in doses which entail less danger.

It was Riess who first, in 1875, applied salicylic acid to the treatment of typhoid fever; and since this first trial, Schroeder, Nathan, Fischer, Ewald, Goldtammer, Bælz, in foreign lands, and in France, Garcin, Noel Gueneau de Mussy, Jaccoud, Oulmont, Hallopeau, Caussidon, and Rabeau, and, especially, Prof. Vulpian, have shown us the advantages which may be derived from salicylic acid in this disease. Salicylate of soda, salicylate of bismuth, and salicylic acid have severally been employed. Salicylate of soda is preferred by the greater part of German physicians, as producing less irritation of the alimentary canal than salicylic acid, and as being quite as good an antipyretic. I do not hold this opinion, and believe with Prof. Vulpian, supported as is this belief by numerous cases occurring in my hospital practice, that salicylic acid possesses—in equal dose—an antifebrile action far superior to that of salicylate of soda; and I am as much a partisan of salicylic acid in the treatment of this fever as I am of the salicylates when it is a question of rheumatic fever. Salicylate of bismuth, which I was the first, I believe, to employ in therapeutics (not, indeed, in typhoid fever, but to combat the fetid diarrhœa of infancy), has been utilized by Vulpian in the treatment of dothinenteritis. Guided by the idea—very just, indeed—that typhogenous virus develops especially in the last portion of the small intestine, Vulpian thought of this medicament, which seemed to him likely to reach without any alteration the diseased places in the intestine, and there combat, *in situ*, the development of the infectious organisms. But the results have not met his expectations; salicylate of bismuth has, indeed, lowered the temperature, but it has had no influence on the march of the disease. So, despite the more satisfying results which Desplats has obtained, this medication has not found much favor.

Salicylic acid should be administered in capsules, and in a dose not exceeding sixty grains, for more than this determines, especially in females, cerebral excitation, buzzings in the ears, and gastro-intestinal irritation. I, moreover, am always careful to give at the same time a little milk to mitigate the symptoms of the latter, and when I come to speak more particularly of the details of management of this fever, I shall tell you the rules which I follow in giving salicylic acid. In doses of from half a drachm to a drachm it lowers the temperature two or three degrees without much influencing the pulse. By not exceeding this quantity of the medicament, I have never observed any cardiac or nervous symptoms to follow. It is not so with the next antipyretic of which I shall speak, viz., carbolic acid.

Desplats, of Lille, was the first to found the *phenic medication* of typhoid fever; the trials made previously by Pecholier, Tempesto, and Skinner were not

successful, by reason of the small doses given. Desplats gives phenic acid in lavements containing twenty to thirty grains, and gives three or four of these a day; these lavements should be retained so that the medicament may be absorbed. Phenic acid has a considerable antipyretic action, and for my part I have seen doses of less than half a drachm administered in lavements, produce a fall in the fever of nearly five degrees. Such an antipyretic action is not, however, produced without danger; it is, in fact, accompanied with profuse sweats, pallor of the integument, and often an alarming state of collapse.

Last year (in 1882), I often had recourse to carbolic lavements, and frequently observed pulmonary congestions in patients thus treated; in calling to mind the toxic effects noted in animals poisoned by phenic acid, where these pulmonary congestions are the rule, I attributed to my medication a certain part in the production of these thoracic complications, and discontinued the employ of these phenic lavements. Since then, at the Medical Society of the Hospitals, at the time of the discussion which followed the report of Ferrand on the method of Desplats, several of our colleagues, and in particular Siredey and Dreyfus Brissac, mentioned similar facts; therefore, Gentlemen, while recognizing the powerful antipyretic action of phenic acid, this medication must be considered as dangerous, and when you have recourse to it you cannot exercise too great care and watchfulness. For my part, I think it should be abandoned altogether.

Resorcin has been little used in typhoid fever. You will see, when I come to speak of intermittent fever, that it has been used to advantage in this fever, but although I have made many attempts to introduce this substance into the therapeutics of our country, I have not obtained any very positive effects from it in ileo-typhus. As for kairine, I am not aware of any definite results which have been obtained from it in this disease.

(To be concluded.)

ORIGINAL ARTICLES.

THE TREATMENT OF FRACTURES OF THE PATELLA BY THE PLASTER-OF-PARIS SPLINT.[1]

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IN presenting this subject to the Society this evening, I wish to describe the method of treating simple transverse fractures of the patella with plaster of Paris, which I have used since 1863, and which has never before been described. I am also anxious that it shall serve to draw out a full discussion upon the treatment of this important injury.

The plaster-of-Paris *bandage* has been more frequently used, and the results have not been sufficiently satisfactory to prevent Hamilton from condemning it in these words: "Plaster of Paris is, of all the forms of immovable dressings, the worst, because it is the heaviest; but of them all it must be said that they are necessarily cumbrous as a form of portative apparatus; they are, to some extent, dangerous, especially in the hands of inexperienced surgeons; they are inefficient as a means of approximating the fragments; they actually serve but one single purpose, namely, to keep the limbs straight; and this they do too effectually in many cases, causing an unnecessary degree of passive ankylosis. The limb can be maintained in a straight position by a much simpler and lighter dressing than a plaster-of-Paris splint, and by means which permit it to be daily examined and the condition of the fragments noted and corrected, and which will allow slight passive motion occasionally to the knee-joint, a practice which has been found in my experience perfectly safe, and useful in some measure, as far as the ankylosis is concerned. In short, to apply the plaster of Paris, and permit the patient to go about on crutches, as is generally acknowledged by its advocates, is to abandon practically every acknowledged indication of treatment, except straighening the limb and securing immobility at the knee-joint."[1]

This criticism, from so high an authority, has evidently been drawn out by his observation on the treatment of the fracture in question by the plaster-of-Paris *bandage*. It will, perhaps, be best for me to state at the outset, in order to avoid a misunderstanding, that I always make a distinction between the plaster-of-Paris *bandage* and the plaster-of-Paris *splint;* two entirely different methods of using this material. The method which I propose to describe is by the use of the plaster-of-Paris splint, which was first introduced by me in 1861, and first applied to a fracture of the patella, in 1863, in a patient of the late Dr. Tucker, of this city, and which I have used in all the cases that have come under my care in St. Luke's and St. Vincent's Hospitals, as well as in my private practice.

Immediately after the receipt of the injury, I elevate the limb slightly, and place it on a pillow, or a single inclined plane, and wait until the swelling and inflammatory action which follow have subsided. The limb is placed in this position simply for the comfort of the patient, and not for the purpose of relaxing the quadriceps extensor muscle, and thus preventing the separation of the fragments, which was formerly considered necessary. Although I have often attempted, I have never been able, to demonstrate that it made any appreciable difference in regard to the separation of the fragments whether the limb was in a straight position or the thigh flexed on the pelvis.

Sometimes, when the effusion into the synovial cavity is great, I apply pressure as soon as the patient is able to bear it, by means of a bandage. When the swelling has subsided, which takes from five days to a week, the following dressing is applied: A posterior splint is made of two thicknesses of bleached Canton flannel, strengthened in the middle, under the knee, by two extra layers; this

[1] Read before the New York Surgical Society, March 11, 1884.

[1] A Practical Treatise on Fractures and Dislocations, by Frank H. Hamilton, 1880, p. 522.

is made long enough to reach from a little above the ankle to above the middle of the thigh, and wide enough to cover two-thirds of the circumference of the limb above and below the joint, but at the joint it should only just cover the condyles of the femur. Two pieces of Canton flannel, of from two and a half to three inches in width, double thickness, one long enough nearly to encircle the limb at the ankle, the other to encircle it at the upper third of the thigh, are prepared at the same time. The pieces designed for the posterior splint are then thoroughly saturated in a mixture of plaster of Paris and water, taking care that the mixture is not too thick,[1] and then smoothed out upon a board with the hand, and applied smoothly to the limb. Then the two bands are prepared in the same way and applied around the upper and lower extremities to hold it in position. A dry roller bandage is then firmly applied over all, and the plaster allowed to set.

As soon as this is accomplished the bandage is removed, and we have a firm posterior splint, secured above and below by transverse bands.[2] Two other strips, of a double thickness of Canton flannel an inch wide, and long enough to overlap on the posterior surface of the splint, are saturated in a fresh mixture of plaster of Paris and then tightly applied above and below the patella, while the fragments are held in position by an assistant, in the same manner as adhesive straps are used for coaptation in this fracture. A dry roller bandage is then rapidly applied with the figure-of-eight turns over the strips. The surgeon then, with thumb and finger of each hand over these coaptation bands, forces the fragments into close approximation,[3] and holds them

FIG. 1.

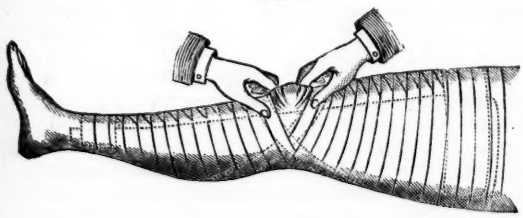
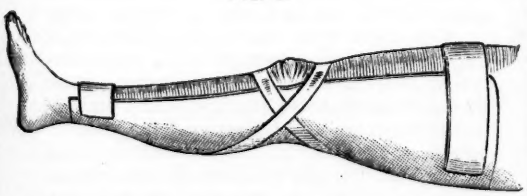


FIG. 2.



there until the plaster has set (Fig. 1). The bandage is then removed and a fresh one applied over the whole length of the limb.[1] The dressing is then complete. Fig. 2 shows the splint with the bandage removed.

It is a good plan for the surgeon, before applying the coaptation bands, to see that the fragments can be easily approximated. In a number of cases I have found some difficulty in keeping the fragments in the same plane, or in preventing them from tilting, there being a tendency for one to rise above the other. This can be overcome by making pressure with the fingers over the line of fracture while waiting for the bands to harden.

This dressing differs essentially from all others, in that the fragments are adjusted by the hands of the surgeon, and the "setting" of the plaster keeps them in the exact position in which they were held.

With this dressing the patient is not compelled to keep his bed, but may sit up or go about on crutches with but little inconvenience.

This apparatus, like all plaster-of-Paris splints, should be applied directly against the skin, care being taken, however, to remove the hair, or else smear the limb with vaseline or oil.

The condition of the fragments can now be examined at any time by simply removing the bandage, and, in case any separation has taken place in consequence of the shrinkage of the limb, it can be corrected by removing the coaptation bands and applying new ones. Care should be taken, if this becomes necessary, which is seldom the case, to moisten the posterior splint in order to insure the adherence of the new pieces.

This overcomes one of the objections urged by Dr. Hamilton—the inefficiency of this dressing as a means of approximating the fragments. Another objection which he makes, in regard to the weight of the splint, is not at all applicable, as patients have never complained of this, and I doubt very much if it weighs any more than the apparatus he recommends.

Pressure sores have never been produced in my experience, nor have the patients ever complained of any pain caused by undue tightness of the dressing. In fact, constriction of the limb by the splint, bands, or bandages, so as to interfere with the circulation, cannot occur, even in inexperienced hands. In this respect it is safer than the plaster-of-Paris bandage which Hamilton so justly condemns.

In order to prevent a rough edge at the upper and lower extremities of the splint, it is advisable to fold them over about half an inch, thus bringing a perfectly smooth edge in contact with the soft parts.

This dressing should be left on for from six to eight weeks. The majority of patients rarely have any appreciable separation of the fragments at the end of the treatment, but as the union is generally ligamentous, a certain amount of separation will take place in time, as in all cases in which there is not bony union.

A case that I treated ten years ago, by this method, came under my notice again a few weeks

[1] Superfine or dental plaster should be obtained. The mixture should be about the consistency of cream.

[2] Sometimes I apply a third band between the knee and the lower one.

[3] The lower coaptation band holds the lower fragment fixed, and at the same time enables the surgeon to make counter-pressure while he forces the upper fragment in position.

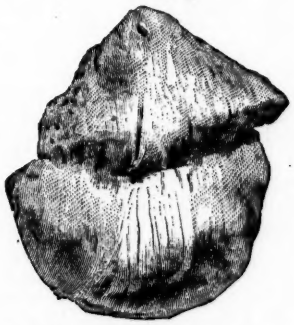
[1] I have sometimes applied a plaster-of-Paris bandage over the splint. This makes a very strong dressing, but it prevents the inspection of the fragments during the course of the treatment, for this reason I prefer the dry muslin bandage.

since; the fragments, which after the treatment were almost in direct contact, I found had separated only a little more than half an inch.

Two cases treated by this apparatus, at St. Vincent's Hospital, resulted in bony union. The first case, a man, about forty years of age, was treated for a transverse fracture of the right patella.

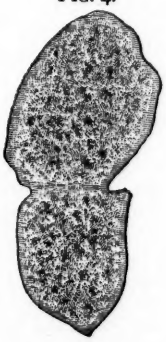
After the apparatus was removed no separation or motion of the fragments could be detected. He remained in the hospital about three months. Six weeks after he left, he was brought back with a severe injury of the head, from which he died. The patella was removed, and was found to have united by bone (Fig. 3). The specimen which I show the

FIG. 3.



Society has been carefully examined, by section, by Prof. W. H. Welch, of Bellevue Hospital Medical College, and he states that it is true bony union (Fig. 4). The line of fracture is slightly oblique,

FIG. 4.



from above downwards and outwards. The lower fragment is somewhat tilted, and overlaps the upper a little; it is also displaced outwards one-quarter of an inch.

This interesting specimen belongs to the late Prof. James R. Wood's collection in the Bellevue Hospital Museum.

In the second case, treated by Prof. F. S. Dennis, although the patient was eighty-six years of age, no separation or movement of the fragments could be detected when he left the hospital.

In comparing this method with the one of wiring the patella in simple fractures, under antiseptic precautions, which has lately come into vogue, I am inclined to give the preference to the one just described, or to any other form of dressing which results in close ligamentous union. The patient is not submitted to a surgical operation which may endanger his life or the usefulness of his knee-joint, for, with the utmost care in antiseptic measures, there is still the possibility of serious complications. I think all surgeons will agree, that a moderate amount of separation of the fragments by a ligamentous band, does not in any way interfere with the usefulness of the limb. Hamilton says "that if the ligamentous band is not more than an inch in length, the use of the limb is not impaired." In a case which I presented at one of the meetings of the Society about two years ago, a patient, aged twenty-two, broke his patella transversely below its middle. He was treated by me in St. Luke's Hospital, and was discharged with a ligamentous union of less than half an inch. Six months after, he slipped in walking and fractured the same patella about half an inch above the first fracture; this also united by ligament. In this case the ligament proved itself stronger than the original bone.

If, then, ligamentous union of a moderate length does not impair the usefulness of the limb and is as strong, if not stronger, than the original bone, why should we submit the patient to any serious danger, in order to obtain bony union, which at the best is of questionable utility?

While bony union, obtained by wiring, may be the goal for which the idealist strives, I question whether it can ever be conscientiously adopted by conservative surgeons.

THE TREATMENT OF TYPHOID FEVER.[1]

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IN rising to open the discussion of the treatment of typhoid fever one cannot help being impressed with the fact that he is approaching a subject the importance of which is not surpassed in the entire range of therapeutics. Very few diseases require such unremitting watchfulness or as careful and discriminating judgment, on the part of the practitioner, as the subject of our discussion this evening, and in very few diseases are we able to see the direct effect of therapeutics with more precision and certainty.

Inasmuch as I am to be followed this evening by men whose researches and experience, in relation to this disease, have already made them known throughout the medical world, men whose remarks will give dignity and weight to the proceedings of this Society long after the cause of its existence shall have been forgotten, I will occupy a small portion of your time only, and will direct my remarks chiefly to some of the salient points in the treatment of the more important symptoms of typhoid fever, leaving the elaboration of many

[1] Read before the New York County Medical Association on March 17, 1884.

details to the subsequent discussion, to which, I feel sure, you are all looking forward with eagerness.

It seems scarcely pertinent to allude to the prophylactic treatment, inasmuch as its nature is so readily deducible from the paper to which we listened with such interest at our last meeting.

I shall not occupy your time with any allusions to the subjects of sanitary plumbing and drainage, consideration of them having been so recently brought to your notice. As physicians, it becomes our imperative duty to prevent drains from becoming infected, by properly disinfecting all the discharges from typhoid fever patients immediately upon their being passed into the bedpan. Perhaps the best disinfectant to use, considering its cheapness and efficiency, is corrosive sublimate. It possesses the advantages of being cheaper and more effective than carbolic acid for this purpose, and it does not stain the bedclothes as does the solution of sulphate of iron. We know that the stools of typhoid fever patients are not infective immediately upon their being passed, but observations are still wanting to determine the exact time at which they become so. It is known that the disease has been spread by stools that have been kept twelve hours for the inspection of the attending physician and then promptly disinfected, but more definite information than this is still lacking, so far as I am aware. The length of time that the stools retain their efficiency as bearers of the contagion seems to be practically indefinite. Two years after the burial of fever stools in a dung-heap several men engaged in removing the dung-heap sickened with the disease. The danger of infecting drains in the post-mortem room is one not commonly considered, but it is certainly important to disinfect the intestinal contents upon the autopsy table before allowing them to reach the sewer. I am not aware that the disease has ever been spread by neglect to observe this precaution, but it seems fair to suppose that it might thus be spread.

After the disease has been contracted our first precaution should be to put the patient to bed at once, and keep him continuously there until convalescence has been fully established. The evil effects of a neglect of this measure are seen with unfortunate frequency in our hospitals, it being a matter of common observation with hospital physicians that cases coming under treatment late in the disease are more liable to exhibit aggravated conditions of meteorism, gurgling, tenderness, diarrhœa, bronchitis, pneumonia, buboes, boils, as well as increased tendency to relapse, to prolonged duration and all the severe complications, and to death, than are those who come under medical care early in the disease. These facts are well shown by an analysis of cases in the reports of St. Bartholomew's Hospital for the year 1882.

The hygiene of the bedchamber next claims our attention. This should, if possible, be large and well ventilated, and should contain two beds that may both be utilized by the patient. The temperature of the room should never be high if it can be avoided, and the patient's covering should be as light as is consistent with his comfort. High temperature of the atmosphere has been shown to aggravate a fever patient's condition, and we see higher degrees of fever and greater mortality in this disease during the summer than at other seasons. All external irritation should be avoided, such as strong light, noises, conversation, and all mental and physical excitement. The service must be quiet and unobtrusive. The patients must never be needlessly opposed, but must be humored in little things, so far as they are not inconsistent with our treatment. A delirious patient must not be forcibly restrained, but must be soothed and quieted in such a manner as every nurse with tact and discrimination understands. I well remember a patient whom I left one day in excellent general condition, but mildly delirious, during the second week of his disease. At night his delirium increased, and the house physician—one of the most efficient house physicians that I have ever had the good fortune to meet—found it necessary to have him restrained by attendants. The services of these attendants were more forcible than efficient, and after midnight in an encounter with them he succeeded in getting out of bed and fell, striking his head rather forcibly upon a hard floor. This seemed to render him furiously maniacal, and he died, exhausted, before daylight. A little more morphine, a large dose of brandy, a little tact, judgment, and good feeling on the part of the attendants might have saved this man's life. In a disease like this, in which trifles seem so often to turn the scale, one cannot be too careful to seize every possible advantage, however trifling, and turn it to good account.

Fever patients generally submit willingly to the taking of their temperature, and I have rarely encountered any objection to taking the temperature in the rectum, which is certainly the best situation for the purpose. English writers seem to have found it difficult or impossible to do this, strange as it may seem to us, and they write in an envious strain of the advantages possessed by continental observers in this respect.

Liebermeister and Friedreich have used calomel in the first week of the disease in doses of seven and a half grains, three or four times a day. They assert that the diarrhœa, which at first is increased by calomel, soon becomes less than before, and that this method of treatment, if undertaken early and systematically, seems to control the duration of the disease, lower the temperature, and lessen the mortality. Possibly this drug may remove some of the infecting material from the alimentary canal, and thus limit its power of producing local inflammatory changes. It is also asserted on high authority that the administration of three or four drops in a glass of water of a solution made up of one part of iodine, two parts of iodide of potash, and ten parts of water, if long kept up will lessen the mortality without marked influence on special symptoms. I have no personal experience with these methods of treatment, but inasmuch as they come to us from authority which we all must recognize, it seems right that they should be given a thorough trial. Doubtless, those who are to follow me this evening can bear testimony as regards this treatment.

The appetite is usually much impaired, and there can be no doubt that the powers of digestion and assimilation are markedly interfered with; but Pavy and Hoppe-Seyler have shown that an active digestive fluid can be prepared from the stomachs of animals in a state of fever. The digestive power varies in fever as it does in health in different individuals, and although some patients may be able to digest beefsteaks—as was the case with a patient of Heubner's, to whom he administered them throughout a severe attack of typhoid fever—there is little doubt that the average patients are unable to do so. Much of the inability to take food in fever is due to the partial or complete arrest of the secretion of saliva and the consequent dryness of the mouth. I have seen a patient's general condition improved by keeping his lips well smeared with vaseline and his mouth frequently moistened by teaspoonful doses of cold water. We should be careful to give an adequate amount of water throughout the disease, and, of course, the desire of patients is, in some conditions, wholly unsafe as a guide in reference to the quantity.

Experience teaches that our main reliance during the pyretic period must be upon milk, and we know that an adult patient will usually do fairly well if he is able to retain and assimilate about two quarts of it a day. Too much reliance cannot be placed upon the stimulating effects of the salts and extractive matters of meat-broths, as seen in their occasional administration. Their effects are of value upon the heart and central nervous system, as are also, in some cases, coffee, tea, and barley-water; but we must look to milk, peptonized or not, as our chief or only food. Rectal alimentation must occasionally be resorted to if the stomach prove irritable; but it must be our constant endeavor to avoid this complication by administering the food in small quantities, and not oftener than once in two or three hours. Two years ago it was my privilege to watch a case of typhoid fever through its entire course and a subsequent relapse, in which milk was not administered at all; in fact, in which substantially no treatment was resorted to. The patient was a pampered child of wealthy parents, always indulged and always self-willed, in whom the various traits developed by utter lack of training, seemed exaggerated by the removal of the slight inhibitory influences to which he was pleased to subject them in health. He would do nothing that it did not please him to do at the moment, nor would he allow anything to be done for him that did not suit his fancy. He resisted all efforts at reduction of his temperature by cold affusions, all efforts at rectal alimentation, and absolutely refused to take anything in the way of food, except beef-juice and lager beer. Occasionally, in my absence, his mother gave him bits of cracker and fruit. On one occasion he prevailed upon her to give him a large banana, which she told me he ate with relish. His struggles against sponging were such that it seemed to me injudicious to insist upon it, and I had the satisfaction, for the first time in my life, of watching a prolonged and well-marked case of typhoid fever substantially untreated through its entire course. His temperature remained between 102.5° and 104.5° for nearly two weeks, but he finally convalesced and made a complete, though tardy, recovery.

As to the use of spirits, we find the teaching of writers widely different. My own plan is to administer it early in the disease only to drunkards or those always habituated to it in large amounts. My belief is that many cases never need alcohol at all; but that a majority are benefited by it in the latter part of the second and third weeks, and often later. It seems to me that the tendency in New York is to begin the administration of alcohol too early, and to measure its amount rather by the actual quantity given than by the effect produced. The pulse and tongue, and the nervous symptoms should be our guides, and of these chiefly the pulse. If the heart's action become feeble and remain so, alcohol is indicated, and the amount to be given can only be measured by the effect produced. In some cases a few ounces of whiskey, in others nearly a quart in the twenty-four hours may be required. Digitalis is often of use under these circumstances.

As the disease subsides, we are compelled to determine exactly when a return to solid food may be allowed; and to ascertain how to answer this question, we must ask ourselves another, namely, What dangers do we encounter by administering solid food during the fever and immediately after its cessation? This depends much upon the character of the food employed. During the progress of the fever, stomach digestion is certainly much interfered with. It has been shown that in dogs affected with septicæmic fever the digestive fluid obtained from the stomach was far inferior in its powers of digestion to that of healthy animals; but that if hydrochloric acid were added, then the digestive powers were equal to those of health. This corresponds with a clinical observation that was made in one of Prof. Kussmaul's wards while I was a student of medicine in the University of Strassburg. The stomach of a man who was under treatment for chronic gastritis was being daily washed out, and the washings were carefully examined in the laboratory of Hoppe-Seyler from day to day. While under treatment of this kind, the man contracted typhoid fever in the hospital, and immediately on the temperature rising above the normal, all trace of free acid disappeared from the washings of his stomach. These experimental facts coincide with the results of clinical experience, and teach us that acids are often indicated for fever patients, and that purely albuminous food is not well borne in fever; but are there any facts which justify us in withholding such food after the temperature has become normal? We have been accustomed to hear that an early return to solid food is liable to produce a relapse; and this has been taken up and repeated by writer after writer, until at present many of us accept it without question. So far as I can learn, there is no clinical evidence in support of such a view. The statistics of a recent epidemic in London, as shown by the cases treated at St. Bartholomew's Hospital, bear strong testimony against this view.

There can be no question that solid food, passing undigested over the ulcers before they have lost

their sloughs and begun to granulate, has produced dangerous complications, such as perforation and hemorrhage; but after the base of the ulceration is covered by granulations, even this danger is removed. I am well aware of the fact that there are no signs upon which we can rely to indicate the depth, size, or general condition of the ulcers. Muscular twitching, of which so much has been said, is not a sign of ulceration at all, it being often absent in cases which go on to perforation, and being less frequent in typhoid fever than in typhus, in which latter disease, as we know, there are no ulcers at all. In view of all these possibilities, however, can there be any objection to allowing a little meat or egg or other food which will be digested *in the stomach* as soon as the fever has subsided and the gastric juice has again become capable of performing its function? I think not; and for some years it has been my practice to allow small quantities of finely divided meat—chiefly beef—as soon as the temperature has become normal. Thus far I have seen no harm result, and I think, on the contrary, that convalescence has been thus accelerated, and the patient's comfort promoted. My attention was first directed to this by an experience which occurred some years since in this wise: An assistant in my laboratory, a third-year medical student, contracted typhoid fever, and was treated in one of the wards of a well-known hospital. After his recovery, he assured me that he returned to solid food two weeks before the physicians were aware of the fact; and in response to further questioning from me, he told me that one of the kitchen-maids took pity upon him in his starving condition, and brought him secretly solid food of various kinds from the kitchen. Although aware of the danger of eating it, his hunger was so great that he was unable to restrain himself, and he partook freely without anything but advantage, the doctor who had him in charge meanwhile assuring him that only his rigid abstinence from everything except milk could have brought about so rapid a convalescence.

Cayley relates a similar case in his very valuable Croonian Lectures on typhoid fever, delivered before the Royal College of Physicians in London in 1880—lectures upon which I have not hesitated to draw largely to-night, deeming it useful to bring to your notice valuable testimony from all available sources upon this very important subject. The case to which Cayley alludes was that of a boy, about fourteen years of age, who passed through a severe attack of typhoid fever under his care in the Middlesex Hospital, and whose appetite returned before the temperature became normal. He continued to be restricted to liquid food. He complained to his mother, who remonstrated with the physicians. Cayley endeavored to explain to her the danger of beginning solid food too soon, to which she replied that she would rather see her boy die of fever than starvation. She accordingly took him home, stuffed him with solid food, and he recovered rapidly—much sooner, Cayley thought, than if he had remained in the hospital on restricted diet.

In selecting the solid food to be first administered to a convalescent from typhoid fever, one should of course select, as I have already suggested, a food which can be digested in the stomach. Whether the stomach is capable of digestion or not, the patient's desire for food and his general condition must help us to determine; but what possible harm can come to a granulating ulcer in the intestine by administering beef in small quantities to a patient whose pylorus it is not going to pass?

Of the administration of beef peptones in typhoid fever I have no personal experience. They are recommended by high authority, and it is to be hoped that we may learn something of their utility to-night from those who are present and have used them. I can testify to the efficiency of condensed milk as a food for these patients, having treated many cases under circumstances in which fresh milk was not obtainable.

Let us pass for a few moments to the consideration of the fever as a symptom.

As a rule, we clinicians are prone to disregard the evil effects of prolonged high temperature upon the various organs and tissues of the body, and thus to underestimate the importance of interfering with this symptom, even though we may be quite unable to cope with its cause. Observation at post-mortem examinations teaches us that extensive and serious parenchymatous changes occur throughout the body as a result of the febrile process, either a long-continued fever which has not necessarily been very high, or a fever of short duration but of high grade. It is my belief, founded on numerous post-mortem examinations, that the patient never reaches the end of the third week of an average typhoid fever without the muscular wall of his heart having undergone in places a granulo-fatty change. We find the muscular fibres in many situations in the organ so filled with granular material that the transverse striæ are obscured altogether, and in some cases minute drops of fat are distinctly visible. A similar change under similar circumstances is frequently seen in the glandular cells of the pancreas, salivary glands, liver, kidneys, gastric tubules, and the voluntary muscular fibre. Another change in the voluntary muscles, known as the waxy or vitreous change, was first described by Zenker. All the voluntary muscles are liable to it, including the diaphragm, in which latter situation its seriousness need not be pointed out. Vascular changes also occur, as is evidenced by minute hemorrhages beneath the serous membranes seen so frequently. Such changes, of course, are not peculiar to typhoid fever. I have frequently found them in other conditions of high temperature, even after high temperature of comparatively short duration, as in cases of sunstroke. These lesions, then, are the result of the febrile process, whatever be the essential lesions of which that process may happen to be a symptom. In connection with this subject, Cayley observes as follows:

"It can hardly be doubted that the result of a large number, if not the majority, of fatal cases of typhoid fever is due to these changes. If we exclude those deaths which are directly caused by the intestinal lesions, as perforation and hemorrhage, which, after all, are the minority, the remainder

may for the most part be ascribed to failure of the heart and central nervous system. And of these, failure of the heart, with its concomitant congestion of the lungs, plays the most important part."

Max Schultze has demonstrated, under the microscope, the injurious effects of high temperature upon living cells.

There is a marked diminution in the number of red blood-cells in fever; and the febrile wasting of the body has been estimated by so good an observer as Leyden, in Berlin, in some cases at an average daily loss of seven parts per thousand of the entire weight, this being not merely the wasting of inanition, but being due largely to the increased consumption of the febrile process.

In a paper on the treatment of typhoid fever, one is hardly warranted in devoting so much time to the consideration of pathological changes except perhaps as a means of emphasizing the importance of certain methods of treatment to be presently advocated.

The various clinical symptoms which indicate the presence of these parenchymatous changes are too well known to us all to require more than passing allusion in this paper.

Lauder Brunton, in his valuable lectures on pharmacology and therapeutics, gives us, in words upon which it would be difficult to improve, the pharmacologist's view of these parenchymatous changes. He says: "When the power of the blood to convey oxygen is lessened by diminution of its red corpuscles, the consequent want of oxidation leads to the accumulation of fat frequently noticed in anæmia; and a tendency to obesity frequently appears after severe hemorrhage. The pathology of fatty degeneration indicates its treatment, etc. . . . But there is one way of influencing tissue-change which I must not forget. By simply keeping an animal in a hot chamber for a little while, the tissues decompose more rapidly, and evidence of their waste is to be found in the urine as increased urea. Quicker decomposition is followed by increased combustion, and the temperature of the body rises. Now begins a vicious circle; for the higher temperature itself quickens tissue-change, and the fire supplies itself with fuel. The heat, too, stimulates the cardiac ganglia, quickens the heart, dilates the vessels, and accelerates the circulation. Acting on the respiratory centre, it quickens the respiration, increases the supply of oxygen to the body, and thus fans the flame. But, quick as the respiration may be, rapid as is the circulation of arterial blood, it is, as Wickham Legg has shown, insufficient in many instances to keep pace with the decomposition of the albuminous tissues, the products of waste accumulate, and we find them in the fatty heart of fever patients and in the livers of Strassburg geese. Now we clearly see that, whatever may have led to the increased tissue decomposition and combustion in the first instance, the high temperature itself is a cause of mischief, and must be reduced."

Having thus reminded you of the varied lesions resulting from the febrile process, let me ask your consideration for a few moments of some statistics showing the results of treatment on the so-called expectant plan, which is so common here in New York, before proceeding to advocate the adoption of the antipyretic method.

Statistics of results of expectant treatment:

	Cases.	Per cent. deaths.
London fever hospital in 20 years,	8,000	18.9
" " " " 9 "	590	15.9
Pauper hospital at Homerton,	1,509	16.8

Principal general hospitals in London during the ten or twelve years prior to 1880:

	Cases.	Per cent. deaths.
St. George's,	387	19.6
Guy's,	295	19.3
University,	163	17.7
St. Bartholomew's,	635	16.3
St. Thomas's,	445	15.7
Middlesex,	461	15.6
King's College,	318	12.2
Total for hospitals of London,	14,125	17.8

	Cases.	Per cent. deaths.
General hospital in Vienna,	17,000	22.5
Hospital at Basle, in Switzerland,	1,718	27.3

Generally in principal Continental hospitals mortality varied from 16 per cent. to 25 per cent.

The English Army gives these results for six years ending 1877:

	Cases.	Deaths.	Per cent. mortality.
Home service,	545	131	24.0
Foreign service,	1,383	564	40.7
English Navy for the six years ending 1878,	414	110	25.5

Jaccoud recounted, as will be seen later, last year, at the Academy of Medicine in Paris, that he had collected 80,140 cases treated in Europe on the expectant plan, with a mortality of 19.23 per cent.

A glance at the statistics of our own army is instructive in this respect. I have taken the following facts from the *Medical and Surgical History of the War of the Rebellion:*

White Troops.	Cases.	Deaths.
During the year ending June 30, 1862,	21,965	5,648
" " " " 1863,	32,166	10,483
" " " " 1864,	10,116	4,475
" " " " 1865,	9,739	5,798
" " " " 1866,	1,285	635

This gives a total for these five years in our white troops of 75,368 cases of typhoid fever, with 27,056 deaths, this being a mortality of 35.9 per cent.

During the years 1864, '65, and '66, among our negro troops there were 4094 cases, with 2280 deaths, this being a mortality of 55.6 per cent.

We have thus a most painful showing from all parts of the world, the worst record of all coming from our own army. The mortality from this disease during the four years of war among our white troops was 35.6 per cent. During the year following the war in the same troops the mortality among those suffering from this disease was 49 per cent.

It must be borne in mind that the lower mortality in many of the London Hospitals quoted above is to be explained by the introduction into many of them of a modified form of the antipyretic

treatment, a faint semblance of which exists in some of our own hospitals.

In a paper by Dr. Francis Delafield, published in the *Medical Record* for November 17, 1883, some statistics of our own hospitals are given. Between January, 1877, and October, 1883, there occurred 1305 cases in the following hospitals: Bellevue, Riverside, Roosevelt, New York, St. Luke's, St. Vincent's, St. Francis's, and Mt. Sinai. The percentage of mortality, estimated by years, varied between a minimum of 20.1 per cent., in 1879, and a maximum of 30 per cent., in 1880. As showing the results in the different institutions, it appears that the lowest rate of mortality during these years occurred in the Mt. Sinai Hospital, where it was 17.1 per cent., and the highest in the Roosevelt Hospital, where it reached the rate of 36.8 per cent. In this very interesting paper is given a brief summary of the treatment in these various institutions which has produced these results. From this I extract the following:

"In the New York Hospital many patients are simply put on a milk diet, with the addition of a moderate amount of whiskey, and no other treatment is used. Peptonized milk instead of ordinary milk is thought to be of service. For high temperature, the body is sponged with equal parts of alcohol and water, and sometimes the fluid extract of eucalyptus is given in fifteen-minim doses. Quinine is not much used. Tympanites is treated with turpentine internally, and in stupes over the abdomen. Opium is given when there is hemorrhage from the bowels, or excessive diarrhœa.

"At St. Luke's Hospital, the treatment is the same, except that quinine is sometimes employed to reduce the temperature, and ergotine hypodermically for intestinal hemorrhage. Either opium or chloral is used to control restlessness and sleeplessness.

"At St. Francis's Hospital, if the cases are seen early in the disease, large doses of calomel are given, with the idea of aborting the disease. Quinine is given in large doses to most of the patients. The salicylate of soda or the benzoate of soda is given by some of the physicians throughout the disease. Cold water, in any form, to reduce the temperature, is but very little used. A solution of the acetate of alumina is given to nearly all the patients, to prevent or control the diarrhœa.

"At St. Vincent's Hospital quinine, in doses of two grains every two hours, is given to control the temperature. Cold water is not employed. Opium is used with diarrhœa and intestinal hemorrhage. At the Mt. Sinai Hospital quinine in large doses is given to nearly all the patients. Cold water is not much used, but sometimes the patients are sponged off.

"At Bellevue Hospital the treatment varies in the different divisions. In one division the peptonized milk is much used. Quinine in large doses is given when the temperature reaches 103°, and sponging is also sometimes used. Opium, the bromides, and cold to the head are used for the restlessness. In another division quinine, in moderate doses, is given to most of the patients. For temperature over 103° sponging with cold water, or the Kibbee Cot and sprinkling with cold water are used. Opium is given when needed. In another division carbolic acid (gt. j) and tincture of iodine (gtt. ij) are given every two hours early in the disease. Quinine in ten-grain doses every half hour is given to reduce the temperature. Sponging with cold water is sometimes used. Opium is employed for severe diarrhœa. In another division, occasional sponging, and whiskey and opium when required, are the only treatment.

"At the Roosevelt Hospital, full bathing has been tried in many cases, but now cold sponging is more used. Bismuth and pepsin are given to many of the patients. In all the hospitals, milk, either simple or peptonized, is the regular diet of the patients."

And now, let me ask, Have we reason to be satisfied with the expectant treatment of typhoid fever? Is the mortality, as shown in the figures taken from the books of our own hospitals, low enough to warrant us in being contented with the results of the various modifications of the expectant treatment in use here? Have we possibly among us any who think, with Skoda, that it makes no difference whether typhoid fever patients be treated or not? Before we endeavor, by discussion, to elicit answers to these questions, let me offer a few facts and figures from other sources, giving the results of antipyretic treatment.

(To be concluded.)

PELLETIERINE, A NEW TÆNIFUGE.

BY JOHN L. DICKEY, A.M., M.D.,
OF WHEELING, WEST VIRGINIA.

A RECENT valuable addition to the remedies used against tænia is pelletierine. It is an alkaloid derived from the root-bark and stem-bark of granatum. It was discovered in 1878 by Tanret and was named in honor of another eminent French chemist, Pelletier.

The powder is grayish-yellow in color. The dose is given by one authority as two and a half grains, by another, fifteen. The preparation most largely used is gotten up in a proprietary form by Tanret, under the name of "Tanret's Pelletierine." It is of the color and consistence of maple syrup, and has a sweet and pleasant, but slightly astringent taste. Each bottle contains an ounce, which is the adult dose. An objection to it is the price, three dollars a bottle. Following is a case in which it was used:

Percy M., æt. 10, had been suffering from a tapeworm for three or four years. While the family lived in Cleveland he had been treated by several physicians at different times, large portions of the worm had been expelled, but the head still remained. Over a year ago the family removed to this city and the boy had been treated by a physician, who succeeded in getting large sections of the worm, but not the head. The case came into my hands, and half a bottle of "Tanret's Pelletierine" was administered on an empty stomach, but owing to the impossibility of getting the boy to take a sufficient cathartic we failed to get the whole worm. Several

weeks later, another and more successful attempt was made.

The boy was given a glass of milk only for supper, and the next morning for breakfast he took another glass of milk containing the remainder of the bottle of pelletierine, about half an ounce, without knowing he had taken any medicine. Half an hour later he was given a full dose of compound cathartic elixir, but his sensitive stomach rebelled and the elixir was vomited. A still more palatable cathartic was given, in the shape of half a bottle of citrate of magnesia, and at two o'clock I called and found him on the *pot de chambre*, having passed a large, watery stool and about half of the worm. Without removing him or breaking off the worm, I gave him an enema of about twenty ounces of tepid water and soap-suds, containing a drachm of common table salt. In a few minutes the injection was expelled with more of the worm, and taking hold of it and drawing it gently away, hand over hand, the whole worm was soon withdrawn, the small head and suckers being clearly visible to the naked eye. It measured about sixteen feet in length. Under a microscope, the four suckers and central fringe of hooklets proved it to be a tænia solium.

The advantages of this preparation of pelletierine over other tænifuges are its quick action, and its pleasant taste and easy administration. I had seen it successfully used last winter by Prof. Da Costa at the Jefferson College Hospital clinic, in a case that had resisted all the well-known remedies.

Giving the injection and gently drawing away the worm I consider important parts of the treatment in the above-mentioned case. It is probable that very often tænia are expelled as far as the lower bowel and that a part of the tangled mass is retained by the sphincter, thus giving the head a chance to re-attach itself. The worm did not once break in drawing on it, but was tough and elastic. After a few minutes' exposure to the air, however, it became brittle and broke easily.

MEDICAL PROGRESS.

THE CLIMATIC TREATMENT OF PHTHISIS.—In a paper on this subject, read before the Ulster Medical Society, DR. JAMES ALEX. LINDSAY says that there are several cases in which the climatic treatment is plainly inadmissible.

1. In acute tuberculosis. Here no treatment is of even temporary avail, and the rapid prostration of the patient makes a resort to travel quite out of the question.

2. During acute exacerbations of the chronic malady. Here the wise treatment is to avoid all causes of excitement and irritation, to soothe the patient, and to wait until the disease shall again resume its chronic character.

3. Where the patient's means are insufficient to enable him to travel with reasonable comfort, or where there is an idiosyncrasy which renders travel peculiarly distasteful and irritating.

In regard to favorable cases:

1. Those which approach the remittent type are peculiarly favorable for climatic treatment.

2. The milder constitutional symptoms are, in proportion to the local pulmonary mischief, the more hopeful is the climatic treatment, and *vice versâ*.

3. It is important to inquire whether resort has ever before been had to travel, and with what results. Patients who are braced and stimulated by change of scene, and who possess much mental elasticity, are more likely to gain advantage than those who are depressed and disheartened by absence from home and the association of friends.—*Dublin Journ. of Med. Sci.*, February, 1884.

DOUBLE OPERATION FOR EMPYEMA.—BRAUSER reports the case of a boy, æt. 3 years, who had pneumonia and double exudative pleurisy after scarlet fever. Pus having been found by the exploring needle, the right side was punctured and pus evacuated. Ten days afterward the same operation was performed on the left side and a quantity of pus evacuated. The child was well in two months.—*Centralbl. für Chirurg.*, February 23, 1884.

ANTAGONISM OF PARALDEHYDE AND STRYCHNIA.—At the meeting of the Société des Hôpitaux, on February 8th, PROF. DUJARDIN-BEAUMETZ reported the results of his experiments on this subject.

His experiments were made on rabbits. He has given to these animals twenty-five times as much as a fatal dose of strychnine, without causing death, after having previously given paraldehyde—there were only a few slight convulsions. These results are similar to those obtained by Luton in his experiments as to the antagonism between strychnia and alcohol. It is well known that Luton injected, in a case of nervous derangement due to alcohol, one-thirteenth of a grain of sulphate of strychnine in the morning, and repeated this in the evening, if the patient was not sensibly better. The antagonism between strychnine and alcohol was observed by Morray, in England, before the experiments of Luton. The latter has attempted to show that strychnia is an absolute antagonist to alcohol, for, while it causes the nervous troubles of alcoholism to disappear, it has no influence on the gastric and other symptoms of alcoholic intoxication. In the same way, Claude Bernard showed that large doses of strychnia had no influence on etherized animals. M. Dujardin-Beaumetz stated that Morselli had made experiments with paraldehyde and strychnia on dogs, with the same results as obtained by himself with rabbits. [The first experiments on this subject were made by Cervello, for an account of which see THE MEDICAL NEWS, October 27, 1883.]—*Progrès Méd.*, February 16, 1884.

TREATMENT OF HERNIA BY PERITONEAL AND INTERCOLUMNAR SUTURE.—In a paper on this subject read before the Academy of Medicine in Ireland, on January 11th, MR. WILLIAM STOKES describes an operation which consists in the deep insertion through the opened neck of the sac, and close up to the external abdominal ring, of a carbolized catgut suture or sutures, according to the size, width, and depth of the neck, and this is followed by the approximation or closure of the canal and pillars of the ring by the insertion of two or more sutures of a stronger and more durable material, such as chromicized catgut, carbolized

silk, or silver wire. It is, in fact, a dual system of suture, one being peritoneal and the other intercolumnar. He believes that in the great majority of cases this procedure will be found to be attended with satisfactory results, always provided that rigid Listerian antiseptic precautions are taken during and subsequent to the operation.—*Dublin Journ. Med. Sci.*, Feb. 1884.

NEPHRORRHAPHY.—On February 11, PROF. ANDREA CECCHERELLI performed this operation, in the Surgical Clinic in Parma, for painful movable kidney. This is the ninth operation of this kind, and the second which has been performed in Italy. After thirty-six hours there was no fever.—*Gazz. degli Ospitali*, February 24, 1884.

INTESTINAL RESECTION AND INTESTINAL SUTURE.—REICHEL, at the close of a report of several cases of resection and suture of the intestine, draws the following conclusions:

1. Sutures should only be placed in healthy intestinal structure, and the gut should be free of contents.

2. The indications for resection of the intestine are, gangrenous hernia, intestinal tumors and wounds.

3. At the conclusion of the operation the peritoneal cavity is to be closed by suture of the abdominal walls.

4. The sutured loop should be replaced entire, and then the abdominal cavity should be completely closed by sutures. Drainage of the peritoneal cavity, as well as fixation of the loop, is unnecessary and should not be used.

A most rigorous diet should always be enforced for the first few days after the operation.—*Centralbl. für Chirurg.*, February 9, 1884.

FRACTURE OF THE NAVICULAR BONE OF THE FOOT.—At the meeting of the Academy of Medicine in Ireland, on January 4, 1884, DR. BENNETT submitted two examples of fracture of the navicular bone, which he believed had not been heretofore described. The first and older of the two specimens came into his possession some years ago from the dissecting-room, so that of course he had no life-history of it, and it had since remained in the Museum of Trinity College. This year a second and more characteristic specimen of the injury came into his hands similarly, and without history. He at once observed its identity with the former example—in both the articular surface of the bone which receives the head of the astragalus presenting distinct evidence of united fracture, which traversed in a regular line the upper and outer part of the surface. Turning to the opposite articular surface, which supports the cuneiform bones, one is surprised to find only the most trivial traces of fracture marking the upper margin of the upper and outer part of the facet. In so thin a bone as the navicular, one would expect to find a close correspondence in the lines of fracture, no matter how arising, on its opposite faces; but it was evident that the planes of fracture originating in the hollow depression on the proximal surface had passed with extreme obliquity to the free margin, almost avoiding the distal surface. He concluded that the force which caused the injury in both cases was a degree of crushing which, if more strongly applied, would have produced dislocation of the head of the astragalus upwards and outwards. With the specimen last obtained he had the other tarsal bones, and exhibited them as proof that the injury in this case was limited to the navicular bone, the others being normal, except only the head of the astragalus, which showed slight trace of change in form, such as its articulation with a bone whose surface was altered by fracture would readily induce.—*Dublin Journ. of Med. Sci.*, February, 1884.

TREATMENT OF SINUSES BY THE INJECTION OF OIL OF TURPENTINE.—CECCHINI reports three cases in which the injection of spirits of turpentine into fistulous sinuses was successful. Believing that the non-closure of sinuses is often due to retention of pus, this being caused by the tendency of most of the injections used to coagulate the albumen with which they come in contact, Cecchini was induced to use turpentine on account of its not coagulating albumen, and also on account of its stimulating and diffusive properties. The cases reported were, two of fistula in ano, and one of fistula of the lower jaw.—*Gazz. degli Ospitali*, January 2, 1884.

MILIARY ANEURISMS OF THE STOMACH.—At the meeting of the Société des Hôpitaux, on Feb. 22d, M. GALLARD spoke of miliary aneurisms as causes of fatal hematemesis.

In 1875 he made researches on simple ulcer of the stomach, and in all the fatal cases he found an anatomical lesion large enough to account for the fatal issue. But in 1876 he encountered two cases of hematemesis, which caused sudden death, with no other lesion than ulceration of a miliary aneurism of one of the branches of the coronary artery. These cases appear to be unique. In January, 1884, he had a similar case—that of a man, æt. 48 years, who was brought to l'Hôtel Dieu on account of hemorrhage from the stomach. There were no special symptoms; the patient simply complained of loss of appetite, and slight gastric pains. Physical examination showed only distention of the stomach. In spite of rational, active treatment, the patient died the next day.

At the autopsy the intestinal tract was found filled with blood; the mucosa of the stomach was violaceous and slightly softened. In the greater curvature of the stomach, on the left near the cardiac orifice, was a small ovoid tumor, as large as an apricot, perforated at the top and on the line of a bloodvessel. There was no doubt that this was an aneurism. A single similar case was published in 1878, by Douglas Powell, in the *Transactions of the Anatomical Society of London.*—*L'Union Méd.*, February 26, 1884.

ABSCESS OF THE SPLEEN; ASPIRATION.—PARZEWSKI reports the case of a man, æt. 45 years, who, two years previously, had an attack of relapsing fever. Three weeks after his recovery he noticed a swelling on the left side, over the situation of the spleen. When an examination was made, Parzewski diagnosticated left pleurisy with effusion, and also found a fluctuating tumor over the spleen. Exploratory puncture with a Pravaz's syringe showed a dirty reddish-brown matter, containing altered blood-globules and detritus. In the course of five weeks three aspirations were made, by which about one pound and a half of the matter was withdrawn each time. The patient was finally cured.—*Centralbl. für Chirurg.*, February 23, 1884.

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"CRANIOTOMY UPON THE LIVING FŒTUS IS NOT JUSTIFIABLE."

THIS is the title of the Presidential Address recently delivered before the Washington Obstetrical and Gynecological Society by DR. S. C. BUSEY. The discussion of this subject is peculiarly timely, since the tendency of obstetric literature has been to urge strongly the claims of the Cæsarean operation, and its modifications, to more favorable consideration than it has heretofore enjoyed. Of course, the subject is presented by Dr. Busey with earnestness and ability, but as to the conclusiveness of the argument, opinions will probably differ.

He holds that "If craniotomy is justifiable, science and the good of mankind demand that the limits of its application should be definitely fixed." Certainly all will agree to this statement, for a general truth—applicable to other operations as well—is expressed. In the greater consideration given of late to the use of the forceps, to turning, induction of premature labor, to symphysiotomy, to the Cæsarean operation and its substitutes, Dr. Busey sees presumptive evidence "of a widespread detestation of craniotomy." But, on the other hand, would it not be quite as just to say that the recent invention of such instruments as the basilyst of Simpson and the basiotribe of Tarnier is evidence that obstetricians recognize craniotomy as a necessary operation?

Again, the author commends the Cæsarean operation because "it may restore to women incapacitated by pelvic deformity the privilege and power of giving birth to an indefinite number of living children"—Lungren having proved that multiple operations are attended by a greatly lessened mortality—and he commends Porro's operation because it prevents subsequent pregnancies, and, therefore, no opportunity is given for craniotomies. But suppose women do not desire an indefinite number of living children, or again, suppose a woman, after having recovered from a Cæsarean operation, becoming pregnant again, chooses craniotomy, and the supposition is just as reasonable as the opposite, is there not an argument against the Cæsarean operation equal in force to that in favor of it?

Again, he states that "there is not, perhaps, living to-day a single obstetric authority of accepted repute who will claim the practicability of craniotomy in cases in which the conjugate is an inch and a half or less, and few hold it is justifiable when the diameter is two and a half inches or less." The statements of some living obstetric authorities do not fully sustain this position. According to Barnes, "craniotomy and cephalotripsy are the means of effecting delivery in cases in which labor at term is obstructed from disproportion, the pelvic contraction ranging from 3.25″ as a maximum, to 1.50″ as a minimum." Delore, referring to the conditions of application of the cephalotribe, says the limits of narrowing within which it can act efficiently are between 2.47 inches and 1.57. Professor Wasseige, in his work *Des Opérations Obstetricales*, Paris, 1881, refers to thirty-five cases of cephalotripsy he had done, with only six deaths; in only two of these could the fatal result be attributed to the operation, and four would have died no matter what operation had been performed; yet in one of the successful cases the conjugate was only one inch and a half. Pajot advises the cephalotribe in all cases of narrowing below two inches, if the instrument can be introduced; only when this introduction is impossible, the Cæsarean section.

In Dr. Godson's paper upon Porro's operation, published in the *British Medical Journal*, January 26, 1884, its distinguished author remarks, "I hope that the time is not far distant when Porro's operation may become one of selection, displacing, to a great extent, craniotomy." This seems a wiser and more practicable position than the one Dr. Busey has taken.

The general voice of the profession is expressed in the words of Dr. Barnes: "A law of humanity hallowed by every creed, and obeyed by every school, tells us where the hard alternative is set before us, that our first and paramount duty is to preserve the mother, even if it involve the sacrifice of the child." With this before us let one compare the table of Porro operations given by Dr. Godson with the result of cephalotripsy in the hands of Wasseige. Dr. Godson's table includes 134 cases, and the mortality of mothers was 55.97 per cent.

But in only 70 of these cases was the condition of the mother "favorable," and these 70 selected cases show a mortality of less than one in three. In Wasseige's cases of cephalotripsy there were 6 deaths in 35, that is, scarcely more than one in six. But 4 who died were unfavorable cases for any operation, and throwing out these, the mortality was less than one in fifteen. Admitting Wasseige's success as exceptional, we may take the results which Pajot had some years ago by his method of cephalotripsy, one death in four, and we still have a less maternal mortality than the best Porro operations. When we add to this professional view of the matter, the fact that women themselves will generally choose the sacrifice of their children rather than undergo any form of gastro-hysterotomy —and in such a matter as this the woman herself must where possible be given the deciding vote— the prospect of an entire abandonment of craniotomy is not encouraging.

RESPIRATORY THERAPEUTICS.

THE stimulus of the prevalent confidence in the Kochian doctrine of the parasitic origin of tuberculosis, has renewed one of those periodic devotions to inhalatory therapy which, since the time of the revered Hippocrates, have followed the discovery of novel gaseous compounds and elements, respirable and irrespirable; and of which the latest revival, a quarter of a century ago, still holds sway in the widespread use of medicated sprays in the treatment of affections of the air-passages.

The personal supervision requisite for effective medication by way of the respiratory tract demands more time than is usually bestowed on domiciliary visits to the sick; and to this may be attributed some of the disinclination of physicians to make use of the air-tract in the ordinary routine of practice. Hence, the method has received much less attention in America than in Europe, especially Continental Europe, where numerous resorts of easy access have been established for its systematic utilization. The great desideratum has been some economic plan of home-treatment, as a fair substitute for treatment at an inhalatorium. Many appliances have been constructed for the purpose. The majority of these being either inefficient, clumsy, or unsightly, they have not met with much favor; and this fact has prevented proper attention from being given to those which possess real merit.

That much good can be accomplished by resort to respiratory therapeutics is well known to those who have given them fair trial; good, too, in many instances, that cannot be accomplished by any other method of treatment. Thus, inspirations of slightly compressed air will distend unused air-cells far more thoroughly and safely than the most violent voluntary efforts; and do it, too, for the weak, as well as for the strong. They will often act mechanically as an expectorant, far more efficaciously than any drug, and, at the same time, improve the appetite instead of impairing it. In many cases of phthisis these results are simply invaluable. Expirations into rarefied air will exhaust emphysematous air-cells of much of their excess of residual air, and by relieving their tension will facilitate the return of elasticity to the lung tissue. This mechanical result alone will often cure asthma of recent standing in old subjects, and even cases of long standing in young subjects. The modifications in intrathoracic pressures, practicable by the use of slightly compressed or slightly rarefied air, are sufficient at times to overcome the mechanical impediments presented in certain valvular and other cardiac deficiencies, and thus favor the ultimate resumption of functions in partial abeyance.

Medicated airs, employed continuously or at intervals, overcome the fetor of bronchorrhœa, and pulmonary gangrene; soothe the irritability of diseased mucous membrane that provokes cough and prevents sleep; saturate the system medicinally with various volatile drugs; and thus do a great amount of good more readily than any other agents. They are the sole substitutes for a balsamic atmosphere within the reach of those who need it and cannot go where it is afforded by nature.

In fact, we cannot ignore the published records of the value of respiratory therapeutics, as collected in monographs and scattered in the journals; nor can we deny to inhalation a respectable position among the healing arts. Should the present impetus to the study of antiseptic inhalations help to place the entire method upon a more intelligent basis, and teach what can really be accomplished in its various modes of use, the revival will have been of service to medicine, even though it fail to find a safe weapon for destroying the dreaded bacillus tuberculosis.

SPURIOUS CYSTS OF THE BELLY DEPENDENT UPON DIFFUSE TUBERCULOSIS OF THE PERITONEUM.

IN the *Centralblatt für Chirurgie*, No. 6, 1884, PROF. KOENIG, of Göttingen, directs attention to a hitherto undescribed affection, which is interesting alike to the surgeon and the pathologist, and which consists of encysted serous exudations in the abdomen, the result of general tubercular peritonitis. The tumors thus formed, and as yet only observed in females, develop gradually, are mobile when small, but fixed when large and in contact with the inner surface of the abdomen, are the seat of undulating fluctuation, and are frequently multiple, smaller cysts being found in the vicinity of the largest one. They are met with at all periods

of life, not alone in subjects of other tubercular affections, but in those who are apparently free from such lesions elsewhere.

That these cases are not absolutely hopeless, is shown by the following example, which also affords a good illustration of the proper mode of treatment:

A previously healthy woman, thirty years of age, had observed for about three months a gradual enlargement of the abdomen, which soon became the seat of intense pain. The median portion of the belly was occupied, from the symphysis to the umbilicus, by a fluctuating, rather lax tumor, which lay in front of the intestines. A free incision disclosed behind the tubercular peritoneum a sac, filled with a slightly turbid, fibrinous fluid, the limiting tubercular membrane of which was excised, and the entire resulting cavity sprinkled with iodoform, after having been washed out with a solution of carbolic acid. A drainage-tube having been inserted, the wound was closed in the usual manner. The recovery was rapid, without any particular signs of reaction, and the woman was perfectly well and attending to her work two years after the operation.

TOXIC EFFECTS OF BONJEAN'S ERGOTIN.

THE large use of ergot at the present time invests the subject of its toxic action with peculiar interest. DR. DEBIERRE, Agrégé of the Lyons Faculty of Medicine, has lately published an instructive example of chronic poisoning, and makes it the text of some valuable observations on the physiological actions of ergot. The so-called *ergotin* of Bonjean is not only an extract of ergot, but a concentrated extract. The patient, a woman of twenty-five, suffering from menorrhagia, took, in the morning, a quantity estimated at seventy-five to ninety grains of the ergotin. The hemorrhage was arrested, and did not recur. At noon, the patient ate some food with a good appetite, and for six hours after the ingestion of the ergot there were no symptoms of toxic action; then, however, severe pains began to be felt in the hypogastric region. A sense of suffocation came on, and urgent demand for more air was made. The mucous membrane of the mouth and throat became exceedingly dry, and the skin was dry, tense, and retracted. The abdominal distress extended to the chest, and severe pain was felt along the course of the œsophagus and bronchi: it had a rasping and constrictive character. In about four hours after the beginning of the pains, tonic and clonic convulsive shocks, having some resemblance to epileptic seizures, were experienced. The respiration was accelerated to 50 per minute, but continued shallow; the pulse was small, soft, and had fallen to 50; and the temperature declined to about half a degree below the normal standard. The surface became pale, cold, and benumbed, and there was a subjective sensation as if the skin were compressing the body with a vice-like grasp. Sensibility to touch, to pain, and to cold and heat declined, and the tips of the fingers, the lips and the tongue, and the bottoms of the feet became anæsthetized, and gradually the anæsthesia extended over the whole body. Vertigo, tinnitus aurium, obscurity of vision, and mental hebetude were experienced; and these nervous disturbances culminated in the epileptiform seizures above referred to.

The restlessness, anxiety, and depression were so considerable, that Dr. Debierre decided to administer ether subcutaneously. The good effects seemed to be little short of magical. The pain ceased, the respiration became calm and easy, the pulse increased in strength and volume, the dyspnœa was no longer experienced, the surface grew warm, and the nervous disturbance subsided. During the following night there was some recurrence of the pain, the dyspnœa, and the præcordial anxiety, but not of the convulsive phenomena. Ether was again given subcutaneously, and to some extent by inhalation; coffee was administered freely, and two doses of chloral, aggregating forty-six grains, were also given. By these means, a decided amelioration of all the symptoms was effected by morning. During the second day, the anæsthesia persisted in a less degree, there were occasional attacks of muscular spasm and of pain, and a strong sense of lassitude was experienced. On the third day of the poisoning, there were still occasional attacks of spasm, and the pectoral and abdominal pains now and then occurred, especially when food was swallowed. On the fourth day the principal symptoms had greatly lessened; vision became clear, the heaviness of the head and the singing of the ears subsided, the constrictive pains in the stomach and abdomen abated materially, the general sensibility returned to the normal standard, and the sense of exhaustion had nearly gone. During the course of the treatment which had effected these results, two drachms and a half of chloral and three drachms of ether were administered. It is an interesting fact, that the most effective means of relief to the symptoms had been the subcutaneous injection of ether. A more exact antagonism has been shown to exist between amyl nitrite and ergot, but this fact was not known, or was not utilized.

The following conclusions seem to be properly deducible from the experience in Dr. Debierre's case:

First, as to the rate of absorption. So many hours elapsed before the symptoms began, that it would seem to confirm the view which maintains that absorption of such medicaments does not occur in the stomach, but in the intestines. In favor of this theory are quoted the experiments of Bouley and

Colin, who found that even large doses of strychnine introduced into the stomach of the horse did not have a toxic effect until the ligature which had been put around the pylorus was divided. In Dr. Debierre's case, the ergot remained a number of hours in the stomach before absorption occurred, contrary to the usually accepted opinion that its action begins in about twenty minutes, and ends in an hour or two.

Second, as to the effects of ergot on the circulation. All the symptoms referable to the circulation indicate that ergot both slows and enfeebles the heart, and contracts the arterioles, but does not increase the vascular tension. This result is in accord with the experiments of Yvon and Tanret, who found that ergot causes contraction of the vessels, after division of the sympathetic, whence they conclude that it acts directly on the organic muscular fibre.

Third, as regards its effects on the nervous system. The cramps and convulsive phenomena, but especially the violent pectoral and abdominal pains, signify spasm of the muscular fibre of the vessels of the bronchi and of the intestinal canal, respectively. To the same action must be attributed the pallor and coldness of the surface, and the subjective sense of chilliness, and, no doubt, also the anæsthesia. The action of ergot on the unstriped muscular fibre admitted, all of the symptoms, indeed, produced by this agent are readily explicable. The slowing of the heart and the diminished acuity of vision, under these circumstances, mean lessened blood-supply to the cardiac motor ganglia and to the retina, respectively. The same action extended to the brain, the observed cerebral symptoms become a necessary result. This theory does not imply, however, as has been heretofore maintained, that ergot stimulates the ganglia and fibres of the vasomotor system.

ELYTROTOMY IN EXTRAUTERINE PREGNANCY.

THE operation of elytrotomy in extrauterine pregnancy is so infrequently performed, that the successful case reported at a late meeting of the Academy of Medicine of Paris, by PINARD, cannot fail to interest our readers. The subject was thirty-one years old, and became pregnant for the third time. This pregnancy was marked by acute pains in the abdomen, inability to engage in her usual avocations, and necessity for remaining recumbent. At the end of pregnancy the ordinary symptoms of labor, though in a less degree, occurred; these passing away, were followed by enlargement of the mammary glands, as after normal labor. At the end of two months the abdominal tumor, which had remained stationary from the time of the symptoms of labor, rapidly doubled its size, and an examination showed that this increase was due to an accumulation of liquid within it; simultaneously acute pains in the abdomen, difficulty in micturition and in defecation, and grave general symptoms appeared. Tarnier being consulted at this time, advised the operation by the vagina, because of the great prominence which the tumor offered in the posterior vaginal cul-de-sac. Accordingly, Pinard incised the salient part of the vagina, and without any difficulty removed the fœtus; the placenta was adherent, and no attempt was made to remove it, but the cystic cavity was injected every two hours with a solution of corrosive sublimate, 1 to 2000. These injections were continued for eighteen days with the same regularity, when the spontaneous detachment of the placenta and its discharge occurred; meantime there was not the slightest indication of mercurial poisoning, or of septic infection. The patient was quite well in a month, showing only a long, red cicatrix in the vagina.

It is stated that only one other case is known presenting the same conditions, and followed by success. But we need but refer to Deschamp's monograph upon extrauterine pregnancy, Paris, 1880, to find two similar cases; one occurring to Schroeder, and the other to an English practitioner, while successful elytrotomy in early pregnancy has been twice done in New York, once by Harrison, and once by Thomas.

PARALDEHYDE.

DUJARDIN-BEAUMETZ, the eminent therapeutist, has recently (*Bulletin Général de Thérapeutique*, January 30, 1884) examined into the physiological action and therapeutical effects of paraldehyde. This investigation included comparative observations with other known hypnotics and anodynes. All the patients taking paraldehyde found it much less disagreeable than chloral. In ten to fifteen minutes after administration, some subjects, especially females, experience a transient dizziness, which is succeeded by a calm, refreshing sleep, lasting from twelve to eighteen hours. No heaviness or headache is felt on awakening; but, when a full dose is taken, the perspiration exhales a strong odor of aldehyde. As a hypnotic, merely, paraldehyde is superior to chloral and opium, but it does not possess the power to relieve pain. In a case of hepatic colic it afforded no relief. It is in insomnia without pain that its utility is undeniable. It has been found to act happily in the insomnia of the morphiomaniac, and in cases of simple wakefulness unattended with pain. These results accord with the observations made in Philadelphia, with which our readers are familiar.

Dujardin-Beaumetz concludes that we have in paraldehyde a useful addition to our therapeutic resources.

SOCIETY PROCEEDINGS.

NEW YORK SURGICAL SOCIETY.

Stated Meeting, March 11, 1884.

THE PRESIDENT, R. F. WEIR, M.D., IN THE CHAIR.

SPECIFIC MYOSITIS OF THE GASTROCNEMIUS.

DR. F. LANGE presented a patient whom he first saw about three weeks ago. At that time there was a tumor in the calf of the left leg. The difference in the circumference between the two calves was at the lower part four and four-fifths inches, while at the tuberosity of the tibia it was about two inches. Some of the symptoms led Dr. Lange to assume that it might be of specific nature; he treated the patient accordingly, and the tumor had diminished considerably in size. He intended to present the patient later in order to see whether the result of treatment will be as he expected. He regarded it as specific myositis of the gastrocnemius, extending as high up as the upper attachment of the internal belly of this muscle, so that it could be traced into the popliteal space behind the hamstring muscles. The initial lesion occurred fifteen years ago. No secondary symptoms followed, but later, the patient suffered from a rheumatic affection, located particularly in the clavicles and sternum. These so-called rheumatic attacks were repeated often, but were always treated successfully by the internal administration of iodine. There had never been any affection of the throat or mucous membranes. For a number of years a stricture has existed, which Dr. Lange had located in the prostatic urethra, and it was yielding to the use of bougies. There was also sclerotic thickening of the skin of the pubis, with central depression and peripheral infiltration. The inguinal and other glands, especially the cubital, were enlarged. There was also a deep depression of the skin of the affected calf, but there had not been any ulceration, as the patient said. Dr. Lange thought that probably there had been a subcutaneous gumma which had undergone softening, with subsequent subcutaneous cicatrization. The scar was pigmented, and presented the characteristic appearance of a specific scar. The patient himself traces the origin of the tumor to a small round nodule, which could be felt beneath the skin five years ago in the neighborhood of the scar, and gradually increased in size. Treatment had been energetic, and consisted of daily inunctions of sixty grains of mercury, and at the same time the patient had taken iodide of potassium and mercury internally, according to Ricord's formula. Since about seventeen days, the tumor had been diminishing in size, and was considerably smaller than when the patient first came under observation. At that time the calf was monstrously enlarged and somewhat œdematous; at its lower portion there existed slight redness of the skin with elevation of temperature. While it could be made out clearly that the tumor belonged to the muscular substance of the gastrocnemius, especially in its inner portion, there was still present a diffuse thickening of the surrounding parts, which made a thorough distinct palpation of the boundaries of the tumor impossible. The circumference of the calf has already decreased by two inches in the lower part, and one inch in the upper, corresponding to the tuberosity of the tibia. The tumor itself can be traced more distinctly as a lengthy, hard mass, which is divided into two portions by a slight depression. This relation could only be made out a number of days ago. At no time had there existed any considerable functional disturbance.

He had also seen a gummatous tumor as large as a goose-egg, which occupied the tendon of the biceps femoris, and under specific treatment disappeared entirely.

THE PRESIDENT said that he had seen a similar infiltration in the sterno-cleido-mastoid, and also in the coracobrachialis muscle, but not of equal size. He asked Dr. Lange if any of the methods by which he determined the location of the stricture in the prostatic portion of the urethra embraced the passage of a full-sized sound to the point of obstruction, with a coincident rectal examination to determine whether the obstruction was in the prostatic urethra. It was the great rarity of the occurrence of stricture in this portion of the urethra that led him to ask these questions.

DR. LANGE said that he had not resorted to the method last mentioned by the President, but that he located the stricture by simply introducing a bougie, which passed in readily up to the point of obstruction, the latter being located so far behind that it must be in the immediate vicinity of the neck of the bladder. The patient, for a number of years, had noticed a diminution in the size of the stream, which within the last few months had been reduced nearly to a dribble.

DR. W. T. BULL presented a case of

RUPTURE OF THE TENDON OF THE QUADRICEPS FEMORIS,

which had been treated by Dr. C. A. Jersey. The man was injured, on Nov. 22, 1883, by having his leg forcibly flexed; he was seen within ten minutes after the injury was sustained, and a dressing of cotton, with a posterior splint, was applied to leg. Twelve hours later this dressing was removed; there was slight swelling around the knee-joint, the anterior surface of the thigh was ecchymotic almost to the groin, and there existed a well-marked depression about two inches above the knee—probably three inches in width. The power of extending the leg was lost. Three long strips of adhesive plaster were passed around the thigh at different points in a figure-of-8 form, making traction in the direction of the knee, approximating the torn edges of the tendon. A Canton-flannel roller was applied from toes to groin, and a plaster splint from ankle over all. The thigh was held at almost a right angle to the body during the application of the entire dressing. The leg was left in this appliance for forty days; after the third week the patient was permitted to go about on crutches. At the end of forty days, the splint having become somewhat softened, a new one was applied, the adhesive-plaster strips and Canton-flannel roller not being disturbed. This second splint was left on for twenty-one days, the patient during the last week being permitted to walk about without crutches. At the expiration of this time the entire dressing was removed. A posterior felt splint, with a bandage from the toes, was worn for one week; then a rubber bandage was applied, which the patient has worn up to the present time. Patient has used a cane until about the tenth of February.

Now—three and a half months from the receipt of the injury—the man has a very useful limb, flexion at the knee being possible to three-quarters its normal extent, and extension perfect and strong. There is much thickening of tissue about the seat of injury, but no sign of the defect beneath the skin usually seen after the rupture of the belly of a muscle.

He had seen several cases of this injury, and in but one was there finally any power of extension whatever, and that was a case in which it existed six months after the injury, and only the fibres of the rectus were torn. As a rule, in an injury of this description the patient loses the power of extension, and he had been led to regard it as a far worse injury, as far as the functions of the knee-joint are concerned, than fracture of the patella. He knew of two persons in whom rupture occurred upon both sides, and both had since been obliged to wear artificial support to enable them to walk.

DR. C. T. POORE had had a case in which rupture occurred on the right side, and there was separation to the extent of about four inches. The patient recovered, and is now able to walk without artificial support, has no limp, and can bend his leg, the only apparent defect being that the extensor muscle is not quite as strong as the other. He treated it by the use of a plaster-of-Paris splint, with a strip of adhesive plaster over the course of the muscle, to hold it down at the point of rupture. There were ultimately about three inches of new tendon formed.

THE PRESIDENT had met with five cases of this injury within two years. In only one was the rupture complete, and he was inclined to believe that in the majority of cases the injury was confined to the central tendon, and that in such cases we had a much better prognosis. In the one torn completely across, which was only seen several years after the accident, there was little or no difficulty in the patient's walking, and no posterior splint was required. All the other cases were treated by the approximation of the divided ends of the muscle by means of strips of adhesive plaster over the thigh anteriorly and fastened below to the sides of the leg, and the use of plaster-of-Paris bandage. He had at present a patient, whom he hoped to show at the next meeting of the Society, in whom both quadriceps tendons had been torn across at the same time, and in whom recovery was nearly complete. In all his cases the injury was the result of muscular force.

DR. JAMES L. LITTLE then read a paper on the

TREATMENT OF FRACTURES OF THE PATELLA BY THE PLASTER-OF-PARIS SPLINT.

(See page 353.)

THE PRESIDENT asked, in presenting the above paper for discussion, what experience, if any, had been obtained by the members of the Society in the treatment of recent fractures of the patella from aspirating the joint for blood effused within it, which had been said to be one of the causes of separation of the fragments. Mr. Heath, of London, and other surgeons had spoken of this method as being serviceable. In the recent discussions which had taken place in Great Britain, arising from Sir Joseph Lister's notable paper on fracture of the patella, the point was made that one of the reasons why bony union did not take place in fracture of the patella was that little fragments of fibrous tissue were caught between the ends of the broken bone. McEwen, of Glasgow, also alludes to this fact, having proved it in one or two dissections. If aspiration allows the fragments to be apposed, it would seem to be sound surgical practice, before applying a splint, to get rid of this interposing small amount of fibrous tissue, by bringing the fragments together and rubbing their surfaces strongly over each other for a few moments.

DR. SANDS doubted whether, in average cases of fracture of the patella, marked superiority could be claimed for any single method of treatment—all methods being more or less imperfect. The treatment recommended by Dr. Little resembled very closely the common one, which consists in placing the limb upon a straight posterior wooden splint, and applying straps of adhesive plaster obliquely above and below the fragments.

He had for many years believed that the extent of primary separation of the fragments depended mainly on two causes, namely, laceration of the fibrous tissues and distention of the knee-joint with blood or inflammatory effusions. He deprecated the premature and forcible attempts often made to approximate the fragments while the joint was distended, believing that such treatment was liable to increase their separation by causing further effusion. He quite agreed with Dr. Little in recommending that, until the subsidence of the swelling, treatment should be restricted to rest of the limb in an extended position, the use of some evaporating lotion, and, when necessary, moderate pressure by means of a flannel or elastic bandage.

He remembered having read many years ago of cases treated in St. Bartholomew's Hospital by simply placing the limb at rest in a straight position upon a posterior splint, approximation of the fragments occurred spontaneously, when the swelling subsided, and very good results were obtained. The uniting medium, although ligamentous, being generally very short, it seemed to him that by the method recommended by Dr. Little, although good results would often be obtained, there might sometimes be found a liability to a tilting of the fragments. Dr. Sands had endeavored to obviate this difficulty by resorting to a method practised by many surgeons, in which strips of moleskin adhesive plaster, attached to the limb above and below the seat of fracture, are brought together and fastened by a buckle directly over the broken bone. Of course, it was presumed that when Dr. Little spoke of approximating the fragments, he meant an approximation of the upper to the lower fragment, as no amount of force was capable of stretching the ligamentum patellæ.

Dr. Sands had also, in some cases, resorted with advantage to Malgaigne's hooks, the points of which were prevented from puncturing the skin by the interposition of several layers of thick adhesive plaster. This practice, he believed, originated with the late Mr. Spence.

He was a little doubtful whether, in bad cases of fracture of the patella, attended with great separation of the fragments, any treatment would produce good results short of a surgical operation. He was convinced, however, that in a majority of cases, many forms of apparatus can be used which would enable the patient to obtain a useful limb.

DR. W. T. BULL said, with regard to the plaster-of-Paris bandage, as distinguished from the plaster-of-Paris splint

in the treatment of this fracture, that he thought Dr. Little had expressed himself adversely to the plaster-of-Paris bandage a little too strongly. It had certainly been thoroughly tested in New York hospitals, and was now generally used in a number of institutions, and he thought the results obtained did not justify the expression that it may justly be condemned.

With regard to this application in the treatment of fracture of the patella, it was the only apparatus which he had used; he regarded it as the simplest form of apparatus that will keep the knee absolutely still, and one which is more easily applied than the apparatus described by Dr. Little. Before applying the plaster bandage he always drew the upper fragment down as far as possible with the hand, and held it there by means of a single strip of adhesive plaster, while a second strip served to "steady" the lower fragment. He had met with a good many cases of fracture of the patella during the last six or seven years, and he had yet to see one in which the upper fragment could not be approximated to within one-fourth to one inch of the lower fragment within a certain time. He said a certain time, because this period varied a great deal according to the quantity of effusion into the joint. He had seen two cases in which the upper fragment could be drawn down directly, and there the plaster splint was applied within a few minutes and allowed to remain until the bone had united by ligament. He had not seen any cases in which the union was entitled to be considered as bony union.

It was suggested, some years ago, that the removal of the effusion could be accelerated by aspiration, and Schede suggested puncturing, washing out the joint, and then treating the puncture with antiseptic dressing. Dr. Bull had not had any experience with the latter method, but he had tried aspiration in two cases. In both he aspirated the joint twice, because the first operation did not facilitate approximation of the fragments, although a small quantity of bloody serum was withdrawn. The second aspiration was about as futile as the first. He had about reached the conclusion that the best method is to allow the effusion to subside, aiding absorption only by firm compression, such as can be made by a firmly applied cotton dressing over the entire limb (laid on a Volkmann's splint), and he had resorted to that measure in cases in which there was a good deal of effusion, and at the end of a period varying from five to ten days, the upper fragment had been approximated within one-quarter to one inch of the lower one with great facility. With such a ligament, if not stretched by flexing the joint too early, the joint functions would be satisfactory.

It seemed to him that no one who had had experience in the use of the apparatus recommended in Dr. Hamilton's *Surgery*, could wish to use it a second time, because it requires constant attention, and the bandages must be renewed every day, or, at furthest, every second or third day. While he believed the apparatus described by Dr. Little is certainly safer for surgeons not experienced in the use of plaster of Paris in the shape of a bandage, he thought the plaster bandage safe enough for any man who has had reasonable experience in the application of bandages in general to the extremities.

DR. F. LANGE had observed a number of cases in which separation of the fragments existed years after injury, varying from one and a half to two and a half inches, and in which the limbs were entirely useful, and the power of extension complete, the patients being able to go up and down stairs without the slightest discomfort. He had treated a number of such cases, and among them were two in which fracture of the patella took place several times, besides one in which separation of the tendon of the quadriceps took place, and in which a gap exists now sufficiently large to allow of the interposing of the hand, yet without functional disturbance of the leg. It seemed to him that after a time the lateral part of the capsular ligaments took up the function of the muscle to a certain extent.

DR. GEORGE A. PETERS had seen one case of fracture of the patella with separation of the fragments to the extent of two inches, and the patient had perfect use of the limb, and was even able to skate.

DR. BULL said that Dr. Little had justly impressed the fact that bony union is not necessary for a useful limb, a fact frequently overlooked by those who have advocated wiring of the patella. He knew of one policeman and one fireman who at the present time are on active duty, and who had ligamentous union of a fractured patella. In one case the ligament was about an eighth of an inch long at the first, but it gradually lengthened until the fragments were separated to the distance of one inch, and yet the man was not aware of the fact.

DR. C. T. POORE asked, if the question of ability to walk was not one of ability of the patient to use the muscle attached to the ruptured tendon. He thought the disability consisted not so much in the separation of the fragments as in the weakness of the muscle which was attached to the upper fragment, and that the difficulty in walking was due to the impossibility for the patient to get accustomed to use the shortened muscle.

DR. LITTLE said that the lower strip of plaster in his apparatus was simply to steady the lower fragment while the upper fragment could be brought into close approximation.

With regard to the difficulty in walking with a long ligamentous union, he thought a distinction should be made between the power of extending the leg and the ability to walk. Patients with a long ligament uniting the fragments are often unable to make complete extension of the limb, but they are able to walk without difficulty. In a recent case in which he applied the splint, at the Post-graduate School, Dr. Powell, who had the case in charge, assured him that he found the patient the day after the accident walking about his room with the aid of a cane. About a year ago, Dr. Stimson presented a case before this Society, in which the patient had had each patella torn from its ligament, and no union had taken place. This man could not extend his legs, but was able to walk about with but little difficulty.

Another point of interest in connection with fractures of this bone, is the liability of the opposite patella to suffer from a similar injury. He had seen a number of cases of this kind. In all these the fractures were caused by muscular action. This is generally attributed to greater strain upon the sound bone and the greater liability of the patient to fall. In two cases which came under his notice, the patients told him that they had suffered a certain amount of pain in each patella before the fracture took place. He had never seen a case in which the fracture was caused by direct violence, that

afterwards suffered from a fracture of the other patella. It is possible that the true explanation of these cases is, that they are caused by an abnormal condition of the bone at the time of fracture. The slight amount of force which caused the fracture in both of these cases, and the pain in the bone felt before the occurrence of the injury, would seem to point in that direction.

DR. BULL asked Dr. Little with regard to passive motion and the advisability of resorting to it, as mentioned in his paper.

DR. LITTLE said that he simply quoted Dr. Hamilton.

DR. BULL said that he had always felt that it was not the right thing to do to resort to passive motion. One should always aim to obtain a short and very strong ligament, and he had always abstained from allowing the patient to move the knee-joint for at least ten or twelve weeks, and by the end of the eighteenth or twentieth week the movements had been satisfactory, and the joint had been strong.

THE NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, March 20, 1884.

THE PRESIDENT, FORDYCE BARKER, M.D., LL.D., IN THE CHAIR.

DR. LEONARD WEBER read a paper on

LOCOMOTOR ATAXIA AND SYPHILIS.

He commenced by saying that although pathologists had not yet succeeded in establishing the existence of such a thing as syphilitic sclerosis of the posterior columns of the spinal cord, there was a considerable amount of clinical evidence in support of a possible connection between the two affections. In former years it had been customary to omit any investigation of the cord as a seat of syphilitic disease, and it had been only within the last thirty years that there had been a change in this respect. In 1876 Fournier published his first communication upon locomotor ataxia as connected with syphilis, and in it he recognized three types of tabes: first, the lumbar, which was exclusively spinal; second, the cephalic type, in which the cerebral nerves were also affected; and, third, the ophthalmic, or amaurotic, type. The subject had also received the attention of a number of other authors, among which were mentioned Hammond and Hutchinson, and within the last four or five years it had been investigated with much interest by German writers. In 1878 Berger's article was published, and in 1879 Erb's first paper, in which he gave the statistics of one hundred cases. Just before the appearance of the latter, Dr. Weber had had the opportunity of talking with its author upon the subject, and it was in consequence of the interest excited in his mind by this conversation that he had written up the cases of locomotor ataxia occurring in his practice during the last twenty years.

Though his statistics were very remarkable, Erb's views were not received with great favor at the International Medical Congress at London in 1881. At an assembly of neurologists last year at Baden Baden, he had reported a second series of one hundred cases of locomotor ataxia, and in no less than ninety-one of them the patients had had chancre and syphilis, while in only nine was this not the case. On the other hand, in twelve hundred patients over the age of twenty-five, who suffered from nervous diseases other than ataxia, only twenty-two and a half per cent. had had syphilis; and, consequently, he was led to announce the proposition that tabes was probably a syphilitic affection, the development and area of which were determined by special causes. In this paper he stated that tabetic women were frequently syphilitic. Among those who distinctly denied the syphilitic origin of ataxia, Dr. Weber mentioned Moxon, Lancereaux, Westphal, and Rosenthal. They claimed that no causative relation had been proved to exist between syphilis and tabes, and that the prognosis, notwithstanding the most active specific treatment, was no better in cases of tabes in which the patients were syphilitic than in those who were free from any such taint.

Finally, in this connection, Dr. Weber alluded to the paper read before the American Neurological Association, in June, 1883, by Dr. Birdsall, of New York, in which forty-two cases of ataxia were reported, with a percentage of nine and a half patients presenting a history of syphilis. In looking at the statistics of this subject then, he said, one could not but be surprised at the vast discrepancy between different authors; the percentage of cases of tabes with a syphilitic history ranging all the way from one to ninety-one per cent. It was reasonable to assume, however, that it was not until recently that much attention had been given to the investigation of disease of the spinal cord in connection with syphilis. In paralytic dementia, some authors gave as high a percentage as sixty-five of cases in which the patients had had syphilis. Sigmund, after reviewing the cases of syphilis which had come under his care between the years 1849 and 1876, came to the conclusion that the disease (syphilis) was on the increase, both relatively and absolutely, and made the assertion that, on account of its multiform character, the gravity of its lesions, and the likelihood of its transmission to posterity, it was the worst affection that cursed humanity. In this opinion Dr. Weber said he could not but concur.

After speaking of masturbation—in females as well as in males—as a predisposing cause, especially in individuals of a neurotic temperament, not only to functional, but to organic disease of the nervous centres, and stating that in a few of his cases of locomotor ataxia it seemed undoubtedly to be the primary cause, he urged the importance in all cases of initial tabes of resorting to thorough antisyphilitic treatment, to exclude specific disease, if possible. In the cases of syphilis in his own practice during the past twenty years, he had not met with very many instances of disease of the nervous system. Some of the cases of locomotor ataxia which he had seen, it was true, presented a syphilitic history; but syphilis did not seem to him to bear the same direct causative relation to the disease that rheumatic trouble did. In tabes, in general, there was often a great difference in the symptoms in different cases, and it was difficult to ascertain the exact seat of the disease, since the neighboring parts, as well as the posterior columns of the cord, were liable to be affected. This was especially true of syphilitic tabes. At the last meeting of the American Neurological Association, Dr. Mills, of Philadelphia, had asserted that sclerosis of the posterior columns of the cord was probably of lepto-meningeal origin. The disease was protean in its pathological and

clinical manifestations, but three principal forms were recognized: first, the typical, as first described by Duchenne; second, the form in which the symptoms were less severe, and might be mistaken for functional derangement but for their persistence; and, third, the atypical, with which syphilis was most frequently associated. No typical case of ataxia had ever yet been proved to be of syphilitic origin, or had yet been cured by antisyphilitic treatment.

As to the diagnosis, the symptoms of specific and non-specific tabes were, for the most part, the same; and as to the prognosis, this was relatively favorable when the syphilitic trouble had not been of long duration, and when the symptoms were irregular in character, and yielded to specific treatment. In regard to the treatment, he could only say that the results had by no means proved equal to what had been anticipated by the advocates of the syphilitic origin of tabes. Still, in a certain proportion of cases, a fair amount of benefit had been derived from it, and the treatment that he advised was thorough inunction with unguentum hydrargyri, and the free use of baths and the iodide of potassium. During the last ten years, his own results had been better than in the ten years preceding, and he attributed this to the fact that ten years ago he gave up the use of the protoiodide of mercury and other preparations of the drug given internally, and since then had almost exclusively employed inunction, in the manner advised by Sigmund. He was confident that anyone who could try this plan thoroughly, would secure quicker, better, and more permanent cures in syphilis in general than by the internal administration of mercury. Occasionally a patient was met with in whose case inunction could not be practised; but such instances were rare. In sudden and alarming manifestations occurring in old syphilitic cases, the iodide of potassium, pushed to the limit of tolerance, afforded better results than any other measure.

Dr. Weber then gave some statistics taken from his own private practice. Of one hundred and thirty-four patients suffering from syphilis, one hundred and fifteen were males and nineteen females, and none were under observation for less than four years; the most of them from ten to twenty years. He gave the history of one of these cases, as it was of interest as being the only instance which had come under his notice in which acute syphilis occurred twice. At the time of the first attack, he placed himself under Dr. Weber's care, and, under the effect of treatment, the disease became latent within two years. He then had no further trouble for three years, when, five years after contracting syphilis the first time, he again had a venereal sore, which was followed by all the signs of acute syphilis, the attack being precisely similar in almost every respect to the first one which he had had. Within two years, the disease became latent; but from 1872 to 1876 he suffered more or less from neuralgic attacks. Of the one hundred and thirty-four cases of syphilis, there were eighteen, or thirteen and a half per cent., in which the disease attacked the central nervous system. In eight patients, six of whom were males and two females, the brain alone was affected; in five (all males) the brain and spinal cord together; and in five, four of whom were males and one female, the cord alone. Of the first eight, two had died of cerebral syphilis, and six were still alive, but not cured. Of the five in whom both the brain and cord were implicated, two had died and three survived; and of the five in whom the disease was confined to the cord, one (the woman) had died of syphilitic hemiplegia, and the other four were still alive.

Of locomotor ataxia, he had the records of seventeen cases. Out of the seventeen cases, three of the patients, or eighteen and a half per cent., were syphilitic. In one the syphilis was contracted after the tabes had existed for a number of years; but in the other two cases it seemed to constitute an important etiological factor. From his study of the subject he had arrived at the following conclusions:

1. There was not sufficient evidence to show that syphilis was the direct cause of locomotor ataxia.

2. There was plenty of proof that syphilis attacked the spinal cord as well as the brain and meninges, and that the disease might sometimes be followed by tabes.

3. Syphilis of the nervous centres occurred sooner in those not treated than in those subjected to a proper course of treatment.

4. Syphilitic nervous lesions were seldom, if ever, cured. Therefore, in order to prevent their occurrence, if possible, early and long-continued treatment of constitutional syphilis should be insisted on.

5. Mercurial inunction, properly carried out, constituted the best means at our disposal for reducing the disease to early and harmless latency.

THE PRESIDENT remarked that in Dr. Weber's cases of ataxia with syphilitic history, the efficacy of specific treatment had not been specially emphasized, and said that he should like to present the history of three cases of his own, in which the therapeutic results were quite marked.

The first was that of a gentleman thirty-five years old, whom he was called to attend in 1875. He had severe pains in the knees and ankle-joints, walked with difficulty, and believed himself to be suffering from rheumatism. He rapidly grew worse, however, until complete paraplegia set in. There was total loss of sensation in both lower extremities, with paralysis of the rectum and bladder; but no mental impairment. Although he confessed that he had been frequently exposed to the danger of contracting syphilis, he denied ever having had a sore on the penis, but stated that he had sometimes noticed a little eruption on his skin and had had sore throat. Under the circumstances, Dr. Barker thought it well to try the effect of specific treatment, and prescribed fifteen grains of iodide of potassium, with one-sixteenth of a grain of corrosive sublimate, three times a day. Soon after, Dr. James R. Wood was called in consultation, and he expressed the opinion that the case was hopeless, but advised no change in treatment. The iodide was then increased to forty grains (no change being made in the dose of the bichloride), and in the course of two months the patient gradually recovered control of the lower extremities, while the paralysis of the bladder and rectum disappeared. He also regained his flesh, which, before, he had been rapidly losing. The treatment was kept up, with the addition of the use of galvanism, and two years afterward he was married. He remained in excellent health until about six weeks ago, when he began to suffer again from pains in his lower extremities. A few days since he was attacked by bronchitis, which was

attended by an amount of prostration altogether disproportionate to the disease; but under the use of doses of fifteen grains of iodide of potassium and ten grains of muriate of ammonia, he was now doing well.

The second case was that of a gentleman, fifty-five years of age, who complained of great difficulty in walking. He had been under the care of a well-known homœopathic practitioner, but on hearing of the result in the above case, had called in Dr. Barker. In 1844, he said, he had had syphilis in Paris, and had been treated by Ricord. He was placed on the same mixed treatment as the other patient, was considerably benefited, and had continued in comparatively good health. The treatment was still resumed at intervals. The third case was that of a physician who had been a surgeon in the Confederate Army, and who consulted Dr. Barker two years ago. His health had been gradually failing for one or two years, there being dulness of intellect, indistinct articulation and difficulty in using the limbs. Syphilis was suspected, although he denied ever having had the disease, and he was placed on fifteen grains of iodide of potassium with one thirty-second of a grain of corrosive sublimate, three times a day. After a fortnight Dr. Clymer was called in consultation; at that time he had paralysis of one side; but within a month the improvement was so rapid that both Dr. Clymer and himself regarded it as something marvellous. He had continued to do well up to the present time. Dr. Barker said that he had mentioned these instances simply for the purpose of illustrating one point, and that was the possibility of getting light in regard to the pathology in certain cases by the effect of therapeutic measures.

DR. AMIDON said that he desired to put himself on record as strongly opposed to the proposition to call typical locomotor ataxia a syphilitic disease. In the series of cases reported in Dr. Birdsall's paper, to which reference had been made by Dr. Weber, only nine and a half patients presented a syphilitic history. The connection between syphilis and ataxia, which certain authorities had endeavored to establish, was based entirely on statistical data; but the unreliability of such a basis was seen in the vast difference of results among the various writers who had compiled statistics on the subject. Our author, as he had been surprised to learn to-night, had put the proportion of tabetic cases with a history of syphilis as one per cent., while Erb in his last hundred cases had reported it at ninety-one per cent.; statistical inferences, therefore, he thought were utterly worthless. The deduction drawn from the results of treatment pointed distinctly to the fact that ataxia was not a syphilitic disease. A good many cases of ataxia of various forms, it was true, had been relieved or cured by antisyphilitic treatment; but never yet a single one of true sclerosis of the posterior columns. Again, the pathology of the disease showed that it was not syphilitic. If it were, it must necessarily be a tertiary lesion, and the tertiary lesions of syphilis were well recognized as of altogether different character from those met with in the affection under discussion. These were well shown in the liver, for instance, in which, whether there was originally a gummatous deposit or cirrhosis, the liver tissue finally became destroyed in the parts affected, and deep cicatricial depressions resulted, which were localized and well defined. The course of the disease also furnished an argument against its syphilitic origin. True progressive locomotor ataxia, as its name indicated, was very slow in its development; while the manifestations of tertiary syphilis were likely to appear suddenly, and rapidly increase in severity. The weight of evidence, he thought, therefore, was decidedly against the lesions of locomotor ataxia being a syphilitic lesion.

DR. R. W. TAYLOR stated that he had already placed himself on record, in the work in which he had been a collaborator with the late Prof. Bumstead, as an opponent of this theory, based simply on statistics of the causation of locomotor ataxia by syphilis. He was, consequently, very glad to see the cautious manner in which Dr. Weber had dealt with the subject. The syphilitic etiology of the disease had been first hinted at by Duchenne, but simply as the result of the fact that he had noticed a coincidence of the two affections in certain instances. He found, however, that antisyphilitic treatment had no effect on locomotor ataxia, and from his study of the subject was led to the conclusion that there was in reality no connection between the two. Until Fournier's publication the profession had looked upon sexual excess, alcoholism, gout, rheumatism, cold, etc., as the primary causes of tabes; but Fournier was struck by the fact, as was indeed very natural, that out of thirty cases of ataxia which came under his observation, twenty-four of them were in syphilitic subjects, and it seemed evident to him that from such a large proportion of syphilitic subjects, syphilis must have a direct causative relation to ataxia. The grounds on which Fournier based the connection between the two diseases were as follows:

1. Frequency of association (as seen in Fournier's cases).

2. The occurrence of ataxia in the tertiary period (by which was understood the third year).

3. Identity of symptoms. (The ataxic symptoms which syphilis produced, however, Dr. Taylor held, differed widely from those seen in true locomotor ataxia.)

4. The benefit derived from antisyphilitic treatment. (In this Fournier stood almost alone.)

5. The symmetry of the lesions. (Syphilitic lesions, on the contrary, were, as a rule, asymmetrical.)

6. No other cause could be assigned for the disease.

Fournier, in his last work, had reported 94 syphilitic cases out of 103 of ataxia; Erb, in his first hundred cases, 61 per cent.; Vulpian 15 out of 126; and Rosenthal 1 out of 65. Here was certainly sufficient disparity to cause doubt in regard to the matter.

There were two other points which could throw more light on the subject, viz., pathology and therapeutics. In the pathological anatomy of syphilis of the nervous system there are found: (1) affections of the vessels; (2) hyperæmia; (3) connective tissue infiltration; (4) gummatous infiltration. These lesions were perfectly well understood. In this distribution it was known that syphilis did not invade a system; its manifestations being in patches, or at random, as it were. The lesions were likely to be of meningeal origin, and scattered more or less asymmetrically. Westphal and Gowers had made autopsies in those dying from ataxia in syphilitic and non-syphilitic subjects, and found precisely the same lesions in both. Erb said very ingeniously that syphilis might produce "a neuropathic state," in which lesions of the nervous system were apt to occur. This expression, however, was a very vague and indefinite one.

In the statistics reported it was painful to read some of the details of the cases (which are for the most part retrospective), given as conclusive evidence of the patients having had syphilis, and the histories were frequently not methodically and critically examined. Yet he (Dr. Taylor) had been called to task by at least one critic in this city for his abrupt statement in the work before referred to, that there was no proof, other than that furnished by statistics, of the syphilitic origin of ataxia, and that the pathological features of the two affections are entirely different.

DR. BIRDSALL said that he came in too late to hear the greater part of Dr. Weber's paper, but he heartily agreed with him in his conclusions, particularly the first and second. The statistics which he had reported in his own paper before the American Neurological Association, he had collected to add to those already recorded in Europe, and he believed that they were the first that had been made in the United States up to that time. Of the forty-two cases of locomotor ataxia given, four were in syphilitic subjects, or nine and a half per cent. Most of these had been under his personal observation, and at the Department for Nervous Diseases in the Manhattan Eye and Ear Hospital the work of carefully recording such cases, and noting the presence or absence of syphilis had been begun long before Erb published his first series of cases. It was a matter of surprise to him that his statistics varied so greatly from those of Erb, though they were not so widely different from those of some other writers. Rosenthal had reported one hundred and five cases, with twelve per cent. syphilitic; Bernhardt thirty-seven, with twenty-five per cent. syphilitic; Westphal seventy-five, with thirty-three per cent. syphilitic; and Gowers thirty-three, with thirty-seven per cent. syphilitic. In Erb's first hundred cases, as had been previously mentioned, eighty-eight per cent. of the patients had had syphilis; and, in his second hundred, no less than ninety-one per cent. Without counting Erb's last hundred cases, the number now reported in all amounted to five hundred and twenty-five, with an average of forty-three per cent. in syphilitic subjects. It was not proper, however, to come to any positive conclusion from such a small number of cases.

He could not agree with the other speakers that statistics ought to be ignored altogether; and yet he thought that inferences derived from them ought to be received with caution. In preparing his own cases for publication, in order to eliminate the possibility of the report being affected by any personal bias, he had submitted the records to the inspection of a gentleman who was not specially interested in the subject, and did not know for what purpose the cases were being collated, with the request that he would go over them carefully and note the various points involved. His conclusions were identical with those at which he had previously arrived. On account of the meagre number of cases, however, he did not regard them as of any special value; and the same was true, he thought, of Erb's series of cases, with their extraordinarily large percentage of syphilitic subjects. Furthermore, in regard to Erb's cases, it might perhaps not be known that this authority did not make any distinction between chancre and chancroid, which would render a modification of the conclusions reached by him necessary in the eyes of all dualists. Still, if it was proved that syphilis was associated more often with ataxia than other nervous disorders, the idea of some connection between the two affections ought not to be ignored entirely. At the same time, he felt positive that up to the present time no convincing proof had been adduced of such a connection; since syphilis was characterized by particular lesions which we know pretty well, and which were quite different from those belonging to progressive locomotor ataxia. Another point—in Erb's cases, as a rule, the syphilis had occurred a long time before the tabetic symptoms appeared, sometimes ten or twenty years. It had been pointed out that in syphilis of the cord, secondary lesions sometimes still remained after gummata and similar troubles had disappeared. Later, it might be that we should have to recognize syphilis, like sexual excess, as one of the predisposing causes which rendered the system liable to suffer from sclerosis of the posterior columns of the cord. In the consideration of this whole question it was important to separate sclerosis of the posterior columns from other troubles, since affections of the lateral columns and other adjacent parts might give rise to symptoms resembling true locomotor ataxia.

DR. EDWARD L. KEYES thought that in the discussion the history, literature, and statistical aspect of ataxia had been sufficiently considered, and said that he would content himself with a few remarks derived from his own experience. In observations upon tabetic patients with syphilis, however, the statistical element could not altogether be done away with. He did not believe that pure sclerosis of the posterior columns was directly caused by syphilis; but, at the same time, he could not see why ataxia might not result indirectly from the effects of that disease on the system. In the course of his practice, he had been called upon to treat a number of ataxic patients which he could not now state positively, but which were certainly not less than two or three dozen. There was one case which he recalled particularly, in which there was a gummatous tumor of the cord, with paraplegia, wasting of the muscles of both lower extremities, and paralysis of the bladder. He became entirely well, so far as muscular weakness was concerned, but was left with all the symptoms of locomotor ataxia, which treatment entirely failed to do away with. The case terminated fatally. At present he had under his care a young gentleman who had had the ordinary manifestations of syphilis, followed by cerebral symptoms. These disappeared under treatment; but now he had commencing locomotor ataxia, for which he could assign no other cause than the syphilis. He had placed him upon pretty active treatment, but did not as yet know what would be the result. He was inclined, therefore, to attach some weight to syphilis as an active factor in the production of ataxic symptoms. Again, he had known of one or two instances in which ataxic symptoms had been distinctly relieved by antisyphilitic treatment. The means upon which he relied were large doses of iodide of potassium and mercurial inunction. It was his practice to commence with the iodide and push it to the point of tolerance. When it produced a depressing effect upon the system he remitted it, and placed the patient on tonic treatment for a time; after which he commenced the mercurial inunction, in connection with hot baths, as was done at the Hot Springs. One case that he particularly had in mind had markedly

improved under this course, though it had not been cured, and he was therefore convinced of the efficacy of specific treatment in certain cases with ataxic symptoms.

DR. A. MCLANE HAMILTON stated that he fully concurred in the opinions expressed by Drs. Birdsall, Taylor, Amidon, and Weber. He thought that the great trouble in this whole matter was that two totally distinct affections had been confounded with each other, viz., classic sclerosis of the posterior columns of the cord (true progressive locomotor ataxia) and cerebro-spinal syphilis. He had found that not more than twelve per cent. of patients affected with true locomotor ataxia had had syphilis; while among the irregular cases of ataxia this was true of a very much larger proportion. In this class of cases, he found enormous doses of iodide of potassium (200 or 300 grains), given in a mild alkaline water, efficient.

DR. F. N. OTIS said that he thought we could hardly afford to disregard altogether the experience of men of such eminent reputation in their special fields as Erb and Fournier, however much we might be inclined to reject statistics, as a rule. These were the two observers with the largest experience, and who had devoted the most study to this subject, considered in its twofold aspect, and we should, therefore, hesitate long before disallowing the evidence which they produced. His own experience, he said, had been small; but his knowledge of the characteristics of syphilis led him to believe that it could invade a system. If locomotor ataxia were a disease of the connective tissue, it acted in the same way as syphilis. Dr. Otis illustrated his point by a reference to the pathology of syphilitic disease of the testicle. Here there was a deposit of gummy material, so-called, which did not in reality differ at all from ordinary germinal matter, and which was capable of forming cicatricial tissue. If the affection were actively treated, the exudation might be taken up; but, if it were let alone, the mass of the testicle would become irregular in outline by the contraction of the fibrous bands formed, and these bands would continue to increase, and strangulate the tissue of the organ. After this cicatricial material began to deposit, no treatment would affect it very much, though a small portion of the testicle might perhaps be saved. In syphilis of the liver, too, there was a very remarkable development throughout of cicatricial material. It had been stated by good authorities that these lines of cicatrization followed the lymph distribution, though this had been doubted a few years ago, because in certain of the tissues, as the bones, for instance, the presence of lymphatic vessels had not then been demonstrated. As before remarked, in cases in which this cicatricial tissue had been laid down there was very little hope of accomplishing anything in the way of its removal by treatment.

Dr. Otis then went on to say that in locomotor ataxia we are ignorant of the pathological significance of the lesions found, and quoted Erb's conclusions as to the limited extent of the knowledge as yet possessed on the subject. Since the true character of the pathological process is still unknown, then, he continued, it would not perhaps be amiss to think of ataxia in connection with such a disease as syphilis, which certainly showed something of the same character in its manifestations. By many it is not regarded as a syphilitic disease because it is not affected by antisyphilitic treatment, but, as was seen in the glance which had been taken at the pathology of this disease in general, when syphilis itself reached the cicatricial stage, no benefit is to be derived from treatment. If it could be demonstrated that there were lymphatic vessels distributed through the cord, by means of which this cicatricial formation might be induced, there would be no longer any difficulty, he thought, in claiming that locomotor ataxia might be distinctly caused by syphilis.

His own personal experience, as he had intimated, is insignificant; but it might, perhaps, be of interest to relate one case that he had met with. Three years ago a gentleman had consulted him who was suffering from tabes, as it seemed to him, and this opinion was confirmed by an eminent neurologist of this city, who pronounced it a typical case of sclerosis of the posterior columns. Already he had a great deal of bladder trouble in addition to the difficulty in locomotion, and the prognosis was regarded as very unfavorable. The treatment recommended was the systematic administration of ergot, carried to as high a point as possible. Afterwards it occurred to him that the bladder trouble might perhaps be aggravated by something located about the genito-urinary apparatus, and on examination he found a contracted meatus. As the want of control of the bladder was the symptom which was the most distressing to the patient, and, indeed, made him continually unhappy, he did not hesitate thoroughly to divide the meatus, in the hope that it might perhaps have some effect, at least, in relieving this, and in the course of a week, much to his gratification, he found that the trouble had so diminished that he now had to use only one towel where eight had been required before. The ergot was continued steadily for two months, during which the urinary trouble continued to improve, but as at the end of this it began to be very badly borne, and as in the meanwhile Dr. Otis had seen the report of the researches of Erb in regard to ataxia, he determined to place the patient on the use of mercury and iodide of potassium. There was no history of syphilis, but he said he had once had a sore on his penis. When he got up to seventy drops of the saturated solution of iodide of potassium, he immediately began to improve. At the present time he is entirely free from the bladder trouble, and is much improved in his gait. In relating this case Dr. Otis said that he had not lost sight of the fact that ataxia not infrequently has its periods of quiescence and of advance. He has had three cases altogether, in which considerable benefit was apparently derived from the kind of treatment mentioned, and in two of them he found that all the urinary trouble was not dependent on the tabes.

As a result of his experience and study, therefore, he felt satisfied that no case of ataxia, if seen at a reasonably early period, should be allowed to go on without the trial of a thorough course of mercury and iodide of potassium, both drugs being carried up to the point of tolerance. In an experience of thirty years he had never yet seen a single case materially damaged by such treatment. The patient suffering from tabes was certainly entitled to the benefit of the doubt as long as there was any question about the true pathology of the disease.

DR. SPITZKA thought that the question was looked at from too narrow a point of view. One important fact all agree to, viz., that in secondary syphilis the disease

undoubtedly affects the cerebro-spinal axis. It has been shown that sometimes in secondary syphilis there is absolute loss of tendon reflex. Why, then, should this not occur in tertiary syphilis? Syphilitic meningitis does not differ at all, in his opinion, from ordinary syphilis. In the position that he had taken, Erb only expressed an extreme view in regard to what is undoubtedly true in the majority of cases. In his own experience he has found that two-thirds of his tabetic patients were syphilitics. In his female cases, however, there was no history of syphilis, but they were only three in number. The disease was apparently ascribable in one case to an attack of scarlatina, occurring after the patient had reached adult life, and in the others to cold. Statistics, as a rule, will show that, in syphilitics, ataxia occurs more frequently than almost any other affection of the nervous system. The proportion of cases in syphilitics, however, is very much the same as of cases of general paralysis of the insane. This disease has been more fully studied on account of the facilities afforded by asylums, and it has been found that the nearer such institutions are to large cities, the greater is the proportion of syphilitics among those suffering from paralytic dementia. In conclusion, Dr. Spitzka denied that tabes was a strictly systemic disease, claiming that the lesions are often found in other parts besides the posterior columns.

NEW INVENTIONS.

ON A NEW FORM OF FORCEPS FOR FACILITATING THE OPERATION OF TRACHELORRHAPHY.

BY JOSEPH PRICE, M.D.,
OF PHILADELPHIA.

ONE of the principal difficulties in performing the operation of trachelorrhaphy successfully, is in making a denusion of the cicatricial surfaces which shall be complete and symmetrical, without impairing the integrity of the two strips of membrane which are to form the canal in the new cervix. To facilitate this portion of the operation, I have devised a pair of forceps, with the intention of using them to cover the portion left undenuded, and at the same time support and throw into prominence the portion it is desired to remove.

The forceps resemble a pair of long dressing forceps, provided with a lock of the ordinary pattern. One blade of the forceps is dilated to a flattened spoon shape, designed to fit the external surface of the cervix, whilst the other is gradually tapered so as to form a much longer and narrower blade. Each blade is furnished with a series of small teeth to fix it securely in the cervical tissues (Fig. 1).

Emmet has so fully described the operation that no subsequent author has added anything of importance to it. The operation is substantially the same as that described when using these forceps, and it is consequently unnecessary to go through its various steps. The uterus is brought down by the double wire tractor and held firmly by an assistant. The amount and shape of the denusion having been decided upon, the lips are held open, and the long blade of the forceps introduced as far as the internal os in the median line of the axis of the uterus on the lower half of the split cervix. It now covers the portion of tissue to be left undenuded. The broad blade fits the external surface of the cervix, and the forceps are locked in that position (Fig. 2). With a broad-bladed scalpel introduced flat along the back of the blade of the forceps, it is now easy to cut a thin slice of the torn tissues of one side the size and shape of the portion to be denuded, clear up into the cicatricial wedge at the angle of the tear. Reversing the knife, a similar slice is removed from the other side.

FIG. 1.

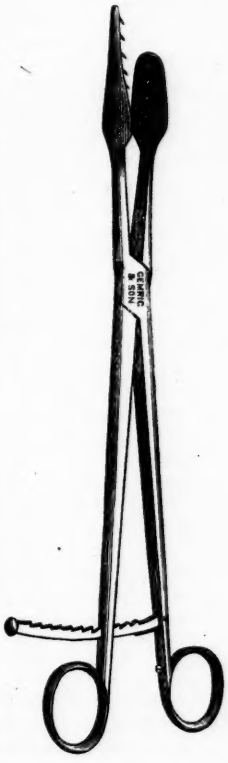

FIG. 2.

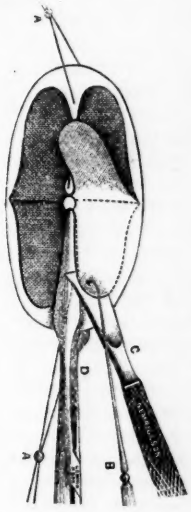

FIG. 3.

The forceps are now removed from the lower and applied to the upper gaping lip, and two similar sections made from it. The two pair of incisions should meet well back from the angle of the tear, removing as much as possible of the wedge of cicatricial tissue, included within the dotted lines (Fig. 3), since, as has been pointed out by Emmet, to its non-removal are due many failures in this operation. Four clean cuts should be made into the angle, and each pair of these should unite in a horizontal line, at a sharp angle, well back from the surface.

I have described a symmetrical bilateral laceration for the sake of clearness, recognizing that the operation would be modified in each case in actual practice by the seat and extent of the tear. In each case, however, the cervical canal would be left undenuded in precisely a similar manner. By the use of these forceps perfect coaptation of the inner portion of the denusion is a necessity, since a band of precisely similar size and shape is protected on each surface, and accidental clos-

ure of the cervical canal is rendered an impossibility. The operation is more speedy, the coaptation easier, and the resistance afforded by the broad blade fixes, to a certain extent, the yielding tissues under the knife.

In the cases in which they have been used the result has been in every way satisfactory, and I have no hesitation in recommending the plan for a more general trial.

123 NORTH ELEVENTH STREET.

NEWS ITEMS.

MUNICH.

(From our Travelling Correspondent.)

THE PATHOLOGICAL INSTITUTE IN MUNICH can scarcely be said to equal those in the other universities of Germany and Austria. The museum is rich in some things, though poor in others. It contains one preparation which certainly is unique. It is a gigantic skeleton, more than seven and a half feet high, the great peculiarity being that the individual who possessed it was of the usual size until his eighteenth year, when he began to grow, the new growth starting from a hyperostosis of the lower jaw. The bones are simply immense; they are not only very long, but more than relatively thick, and covered with osteophyte formations. It is difficult to place this specimen pathologically, whether to regard it as the result of an affection affecting a single system—here the osseous—or to regard the whole, muscles and all parts together, as a true gigantic growth depending on a congenital disposition.

In malformations the collection is relatively poor, not to be compared in this respect with the museum in Prague, the latter, having probably the richest collection in Europe. From some cause or other, Bohemia seems to produce more malformations than any other country, and here, if I recollect aright, the only successful operation on a twin monster was made. The father, a country physician, operated shortly after birth, one of the children surviving the operation.

THE LABORATORIES OF THE HYGIENIC INSTITUTE are all exceedingly well equipped, and several novel contrivances have been introduced. The director of the Institute is Prof. v. Pettenkofer, whom perhaps most of the readers of THE MEDICAL NEWS will know from the publication, in the *Popular Science Monthly*, four years ago, of translations of some of his unequalled popular lectures. One part of the institution is devoted to investigations carried out for private individuals, which investigations are paid for by the individual. The yearly appropriation for the maintenance of the institution is only eight thousand marks, this, of course, being outside of the pay of the professor and assistants, and with this they accomplish wonders. I should have mentioned that the courses here are also largely attended by architects.

BEER.—One is always astonished in Munich at the immense amount of beer that is drank there, especially when it is considered that the beer is so heavy, containing one per cent. more alcohol than most of the beer elsewhere manufactured in Germany. I doubt if people would be able to drink, with impunity, a similar amount of alcohol in any other form—that of whiskey, for example. Yet we are accustomed to think in America that whiskey is the best, purest, and most healthful form of alcohol. In most parts of Germany the beer drinking is all done at night; beyond a glass at the midday meal, few people think of drinking during the day. From the soporific effects of beer one would think it should only be drank when the labor of the day is over, and one wants to bring himself into a pleasant, sleepy frame of mind before going to bed. In Munich they take it with their breakfast, after their breakfast, and, in fact, at all times, and one cannot help thinking, when he looks upon the stalwart figures, and sees how productive they are, both physically and mentally, "What would these men be if they only drank beer at night?"

TENNESSEE STATE MEDICAL SOCIETY.—The fifty-first annual meeting of this Society will be held on April 8th, at Chattanooga.

THE MEDICAL ASSOCIATION OF THE STATE OF ALABAMA will meet in Selma, on April 8th, and will continue four days.

MISSISSIPPI STATE MEDICAL ASSOCIATION.—The annual meeting of this Association will be held at West Point on the 2d, 3d, and 4th of April.

UNIVERSITY OF LOUISVILLE.—The forty-seventh annual commencement of the Medical Department of this institution was held on February 28. The degree of M.D. was conferred on eighty-four graduates.

HEALTH IN MICHIGAN.—Reports to the State Board of Health for the week ending March 15, 1884, indicate that rheumatism has considerably increased, and that pneumonia, whooping-cough, inflammation of the kidneys, dysentery, and inflammation of the bowels have increased, and that scarlet fever has decreased in area of prevalence.

Including reports by regular observers and others, diphtheria was reported present during the week ending March 15th, and since, at thirteen places, scarlet fever at eighteen places, and measles at five places. One case of smallpox was reported in Bath, Clinton Co., on March 15th.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM MARCH 18 TO MARCH 24, 1884.

MATTHEWS, WASHINGTON, *Captain and Assistant Surgeon.*—To be relieved from duty in the Department of the Missouri, and to report in person to the Surgeon-General of the Army for duty in his office.—*Par 12, S. O. 62, A. G. O.*, March 15, 1884.

GIBSON, R. J., *First Lieutenant and Assistant Surgeon.*—Relieved from duty at Fort Hays, Kansas, and ordered to Fort Wingate, New Mexico, for duty.—*Par. 3, S. O. 58, Headquarters Department of the Missouri*, March 18, 1884.

GANDY, CHARLES M., *First Lieutenant and Assistant Surgeon.*—Assigned to duty at Fort Brady, Michigan, as post surgeon. *Par. 6, S. O. 56, Headquarters Department of the East*, March 22, 1884.

CROSBY, WILLIAM D., *First Lieutenant and Assistant Surgeon.*—Relieved from duty at Fort Hauchuca, A. T., and ordered to Fort McDonald, A. T., for duty.—*Par. 1, S. O. 20, Headquarters Department of Arizona*, March 15, 1884.

EDIE, GUY L., *First Lieutenant and Assistant Surgeon.*—Assigned to duty at Fort McIntosh, Texas.

ROBERTSON, REUBEN L., *First Lieutenant and Assistant Surgeon.*—Assigned to duty at Fort Ringgold, Texas.—*S. O. 33, Par. 3 and 4, Headquarters Department of Texas*, March 17, 2884.